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ERRATA.

P. 162, l. 16 from bottom, for "many" *read* "some."
" 167, l. 23 from top, for "best," *read* "test."
" 174, l. 18 from top, for "point," *read* "part."

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

ART. I.—*On the Origin of Species;* by THEOPHILUS PARSONS, Dane Professor of Law in Harvard University, Cambridge, Massachusetts.

IT has frequently occurred in the history of science that some startling theory, which, when first announced, was regarded as the antagonist of received opinions, and became at once the subject of earnest hostility as well as unqualified approbation, has, after much discussion been importantly qualified and modified, and thus reconciled with views which it seemed to contradict; and when thus shorn of its excess and moderated in its demands, has been generally adopted as an important addition to knowledge. It may yet be so with Mr. Darwin's views.

His theory, stated very briefly, is, that all organisms tend to reproduce themselves in a geometrical ratio, and with such exuberance of life, that each one would speedily fill the earth, if not repressed by constant and powerful causes of destruction. Hence but a very small proportion of seeds or ova which are impregnated are able to mature and reproduce. Therefore there must be a competition, or as he phrases it, a "struggle for life," among all these impregnated germs of life; and if one in a hundred only lives there must be a reason why that one lives rather than the ninety and nine which perish. This reason must again be frequently, or at least sometimes, that it had some advantage in this "struggle for life," by a structural or functional difference. That is, it varied from its kindred, in such wise, that it was somewhat easier for it to live, to grow, to mature, and to reproduce, than for them. This difference or variation it must, as a general rule, impart to its offspring. When it be-

came established, the same law of struggle, of advantage, of life, and of death, would operate upon this new and improved variety, and would cause another and a farther improvement. As this law is universal, and must always have operated upon all organisms from the beginning, not only are varieties established in this way, but so likewise varieties become species, species become genera, and so also orders, classes, families are formed, and thus finally we may suppose that all the organisms of the earth, living and extinct, animal and vegetable, have proceeded from the simplest original form of life.

While much interested in Darwin's work and in the discussions and controversies to which it has given rise, it occurred to me to consider whether one of the limitations which he seems to have imposed upon himself, was necessary. He assumes, and reasons exclusively upon the assumption, that the successive changes by which these great results have been brought about have always been minute and slow, and have only become sufficient to reach their consummation, by an indefinite accumulation of effects, through the indefinite periods of time which geology affords them. It seemed to me that this assumption was quite unnecessary, and therefore unphilosophical; and supposing that these changes may sometimes have been much greater, I then inquired what would be the effect of this supposition upon the general theory, that the succession of organized being has from the beginning been produced by generative development. This paper is intended to suggest—and only to suggest—some of the results to which I have come. Upon the question whether I have not departed so widely from the theory of Darwin, that I have no right to use his name, I have nothing to say. I wish only that these suggestions may pass for what they are worth whatever that may be.

To say that it is the tendency of all organisms to reproduce their like, but with some difference, would be merely to utter a truism, for there is almost or quite always some family resemblance between offspring of the same parents, and always so much of difference that no two of the offspring are ever undistinguishable from each other. We may say, however, that one certain law of this difference, or variation, is this; that while a slight difference is universal, great difference is less common, and the greater the difference the more rare it is, and therefore the less to be expected in any given instance. The question then arises, how far this difference may go; or to say the same thing in other words, what limit is there to the possible immediate variation of offspring from their parents and kindred?

The law of variation is itself variable; and while we have little knowledge of the causes of variation, we have none whatever of the limits to which it may be carried. Indeed, if we assume that there must be some limit to the possible extent of

variation, we may infer that it must be a very broad one, from the instances of extreme monstrosity which science has recorded. Let us say, then, that we will assume that there may be as much variation or aberration as these records prove that there has been, and no more.

Perhaps abnormality always seems to us a mischief, and by monstrosity we always understand aberration in a wrong direction; and facts would justify the inference that extreme aberration is usually a degradation. But we have no sufficient reason for saying that this is a *law;* or, in other words for asserting, that there can never be monstrosity in a right direction; or in yet other words, that the aberrance can never be an improvement and a help. As this seems to me an important principle I restate it. We know that it is the tendency of all organisms to reproduce their kind, but with some difference. We know, for all the improvement in our domestic animals proves it, that this difference may be improvement. We know that this difference may be carried to an enormous extent, as a mischief, because the records of monstrosity prove it; and we do not know that this difference may not be carried to an equal extent in the opposite direction of improvement.

My position therefore is precisely this. It is always possible that offspring may be born, differing as much from their parents and kindred in the way of gain, of advantage, and of improvement, as we know that offspring have differed in the way of loss, of hindrance and of degradation; and therefore when I speak of extreme aberration I shall mean by it variation carried to this extent.

Admitting this principle as possible, let us proceed with it to consider what may be called the system of Agassiz; using his name only because he has given to it great development and full illustration.

Take first his assertion that there must have been in each geological age many new creatures; say if you please an hundred or a thousand, and consider this as proved and admitted. Still it leaves wholly untouched the question how these new creatures were created. And be the answer what it may, that answer so far as it is only an answer to this question, leaves the assertion of Agassiz untouched. But if we bring to the question, how were these creatures created? the possibility of aberrant variation of offspring in the direction of improvement, we bring to it one answer. For example: suppose the time to have come when there is to be a new creation, and it is to be a dog, or rather two dogs, which will be the parents of all dogs. How shall they be created? We may say of this either of five things. One is, that we do not know, and never can know, and had better not inquire. This does not seem any answer. A second is, that they will be created "by chance." This also seems to me no answer, because chance is a word only and not a thing. A

third is, that they will be created at once and out of nothing, by the absolute will of a creator. This answer does not satisfy me much better. The fourth is, that they will be so created by absolute fiat, out of a proper quantity of earth and water, with the necessary chemical elements in due proportion, which had been summoned to meet together in a proper place by the will of the Creator for that purpose. But this answer does not recommend itself to my reason much more than the others. The fifth is, he will be created by some influence of variation acting upon the ovum (before or at conception or during its uterine nutriment) of some animal nearest akin—a wolf, a fox, a hyena, or a jackal; and the brood will come forth puppies and grow up dogs to produce dogs. Now the question is not whether this last answer offers a probability *per se*, but whether it is not after all less improbable than either of the other suppositions; less unphilosophical than either of the other answers, and therefore to be accepted on that ground: and I may say in passing, that if the present favorite theory for accounting for the diversities of our domestic dogs, by referring them to four different origins, be adopted, we may then conjecture that each of the four animals above named brought forth its own puppies, to be the progenitors of their respective families.

Let this doctrine of the new creation of new species, by generative development through variation be accepted, and we have Darwin's theory of the origin of species by successive generation; and instead of opposing the theory of Agassiz, it confirms it; because it adopts and reasserts the principle of new creations, and offers some explanation of the way in which they were made.

Let us glance—and only glance—at some facts in geology and zoology, to see what would be the effect of this principle; and I shall carefully limit myself to the most general suggestions, on a topic which would fill more than a volume.

At the beginning of the fossil records of life, in the Silurian formation, we find trilobites of various forms; and recently a Limulus or something akin to a Limulus has been found there. There are other Crustacea; but with these two only, is it not possible to account for all the Crustacea which have ever existed or now exist, without overstepping the rationally possible limits of extreme variation in offspring, simply by arranging those which we already know in a chain of affinity?

But how shall we get to the vertebrates? These same trilobites ran up through all the palæozoic rocks, through the Silurian, the Old Red-sandstone, the Carboniferous and the Devonian, and are lost at last in the Permian. Near their end, when they are already thinning out, we have, in the old red-sandstone formation, the "buckler head,"—or, to use the Greek name given by Agassiz, the Cephalaspis. And we have also the fossil flying fish, or using again the Greek name, Pterichthys. The first of these was long regarded as a trilobite of the genus Asaphus, until Agassiz

at length determined it to be a fish. Of the second, Murchison says in a letter to Miller, "if not fishes they approach more closely to crustaceans than to any other class. I conceive, however, that Agassiz will pronounce them to be fishes, which together with the curious genus Cephalaspis *form the connecting links* between crustaceans and fishes." Now, is it too much to infer from these facts, and always within the reasonable limits of generative aberration, that either of these animals, if a crustacean was so nearly a fish that some of its ova may have become fishes; or if itself a fish was so nearly a crustacean, that it may have been born from the ovum of a crustacean? We may add indeed, that the Eurypterus, now called a crustacean, was regarded at one time, by Agassiz, as a fish.

If fishes may thus have begun to be, and we may suppose that, having begun, they could be so arranged by their affinity and gradual difference as to account in this way for the successive new creation of their kinds, we may then pass to the question of reptiles.

Here also we have Lyell's Dendrerpeton, Owen's Placodus, and the Archegosaurus of von Meyer, all of which were held, and somewhat firmly held, by the highest living authority on this point—Agassiz again—as fishes; and all of them after further and final investigation, have been lifted out of the water by the same strong hand, and placed upon dry land, as reptiles. I know the explanation of this; but does not the fact itself suggest irresistibly that we have here what Murchison calls "connecting links." Links, that is to say, through which, by generative variation, the fish passed into the reptile, and so the family of reptiles began. So too, possibly, the Pterichthys, or fossil flying fish, the Pterodactyle, or huge winged fossil reptile, may suggest the possibility of a similar origin for birds.

As to that difference between vegetables and animals, which some have regarded as the greatest difficulty, I would say only what every one who owns or uses a microscope knows, that the line which separates the protophyta from the protozoa is constantly changing and always uncertain; and that if the organisms which lie along this line, should have offspring which are certainly vegetable, or those which are certainly animal, in neither case would the offspring differ much from the parent.

Nor let it be said that the geological records exhibit numerous instances where a race which succeeds another, does not come into existence until a certain period after the kindred race from whom they might have come has utterly perished. It is not quite so. On the contrary, in most cases, the great classes of animals lap over, as in the instances given, of crustaceous trilobites and the fish found with them, and again the fish and the earliest reptiles, in a way which has always suggested, of itself, this idea of succession by generative reproduction. There are eminent naturalists who read in the records of geology the plain declaration

that there have been some—perhaps many—cataclysmic destructions of whole orders of being, followed by periods characterized by the absence of organic life. If this were *proved*, there must have been not only many new *creations*, but many new *beginnings* of organic life. It must be remembered however, that the geologic record is assuredly not yet wholly unrolled; and that we are not sure that we read aright all that is seen. I have some doubts whether there be an instance in which such an interval of absolute nothingness unquestionably occurs; or one, even in the present state of our knowledge, in which among the races passing away there are not found, and far within the limits of extreme aberration, some who *may* have been their offspring, and the parents of succeeding races.

But I must forbear following these suggestions further. The difficulty of admitting the transformation is, I know, great; and still greater difficulties must be encountered in other parts of this supposed chain of reproduction. A very great one to my own mind arises from those beds below the Silurian, which, on the one hand, are wholly free from traces of life, and on the other, from evidence of destructive alteration by heat. They seem to me to lead strongly to the conclusion of Murchison and others, that the earth had only then become cool enough to make life possible, and consequently that life must have begun there; and there certainly we find it already very various. But, not to insist that with farther knowledge, wider discovery of "connecting links," or transitional forms, and better examination, all these difficulties may be materially lessened, I say at once that I should accept them all unhesitatingly, rather than the notion that the first horse, or dog, or eagle, or whale, flashed into being out of nothingness, or out of a mass of inorganic elements which had been drawn together in due proportion for that purpose.

This last supposition is inevitable if we reject the first.

The one thing I would be understood to assert, is, that science must now *elect* between two hypotheses, which together fill the whole ground, and cannot *both* be rejected. One is, that the animals and vegetables of the world have been formed, by absolute fiat, out of a mass of inorganic materials. The other, that they have come into being successively, by generative production, of some kind and in some way. When Milton tells us that

> *   *   *   *   The earth obeyed, and straight
> Opening her fertile womb, teemed at a birth
> Innumerous living creatures, perfect forms
> Limbed and full grown. Out of the ground arose
> As from his lair, the wild beast where he dwells
> In forest wilds, in thicket, brake or den.
> The grassy clods now calved; now half appeared
> The tawny lion, pawing to set free
> His hinder parts: *   *   *   *

he adopts and adorns the first hypothesis; but while Milton was a great poet, he was not so great a zoologist.

I do not now assert that no creature *can* be made out of nothing, or out of the dust of the earth, nor do I speak of the first beginning of creation; nor of anything but the existing and extinct floræ and faunæ. In reference to the various species of these, I say only that this is the last conclusion which we should adopt, and only when driven to it. Perhaps I may illustrate my meaning thus. If a pair of undescribed mammals, about as large, we will say, as a fox, with young or preparing for them, were found to-day in some district in England which has been thoroughly explored, and of which the fauna and flora were perfectly well known, and these animals differed in some specific essentials from any known animal, there would be a vast amount of speculation about their origin. One writer would say that they had escaped from a menagerie or from some ship; another that they had always been overlooked and undescribed until now; another that they were hybrids, and there would be much discussion as to what animals could have produced them, like that which Gilbert White tells of about the bird which he thought a cross between a pheasant and a hen. There would be no limit to the extent or variety of the discussion,—excepting this. No naturalist would, I think, explain their appearance at that time and place, by supposing that they had been made out of nothing, or out of the dust, suddenly, where they were found. If any one ventured upon this hypothesis, I do not believe that it would be generally adopted. I do but apply the same way of thinking to past times. When the new species appears first in the geological strata, I say that its creation from nothing or from the dust should not be held, until all other possibilities of production are exhausted and rejected. For creation from nothing is just as possible now as it ever was; and we have no reason for saying that it would not be as natural *now*, as likely to occur, and as worthy of admission and belief.

What do we gain by the use, in this connection, of the word miracle in the sense of an exceptional interference by omnipotence? When one of the wheels of Babbage's calculating machine turns up its numbers in a certain unbroken series for a million of times, and then a new element is suddenly introduced, and an old one goes out, this apparently disturbing thing is just as much a part of the machine and its operation as all the rest. The illustration fails so far as this. Babbage calculates his machine and sets it going, and leaves its working to the natural laws which he finds in operation. God never leaves his machine, for if he did it would instantly perish, because it is always his present activity which gives force and efficacy to the laws by which He works.

But what shall we do with that other principle of Agassiz, that all this successive production or creation of new creatures has happened by the will of a creating God; or, to use his own

phrase, that each new creature has come into being by the fiat of the Almighty? What I do with it, is to accept it readily and entirely. For when the voice of God issues the fiat and says let this thing be, is it not as perfectly obeyed although that thing comes into being by generative development, as if it sprang forth from nothing or from the dust?

And again what shall we do with the principle of Agassiz, that in all these new creatures there is no chance and nothing arbitrary, but a coherence and coördination of parts, and a unity of purpose and of place, which prove the whole to be the work of one directing mind and one causative power. Again I answer, admit this also freely and gladly; thankful for every argument and illustration which enforce it. For what is there in the supposition that God has his own laws of divine order, and operates through these laws, and by the means which He has provided, (no matter how universal these laws or how far back the chain of influences or causes extends,) to prevent our recognition of Him and of his wisdom in his works.

But what shall we do on the other hand with Darwin's "struggle for life," and consequent "natural selection," which plays so great a part in his theory? Again I say, if farther investigation renders it probable, as I think it will, admit this also with perfect readiness to play whatever part sufficient evidence may assign to it, be that more or less. The fact *to some extent* is obvious and certain. And may not God act as well through this "struggle for life" as through any other of his laws? Must it be regarded as a blot, an imperfection, which he could not help, and bears with as he may? If we regard it as an instrument, by means of which he works out universal, inevitable, and never ending improvement, incorporating this law with the nature and essence of every thing that lives, or can live, may we not see in this also, at once his infinite love and his infinite wisdom?

Then as to hybridism. Darwin admits the vast preponderance of authority against the continued fertility of hybrids, but still thinks that there are some qualifications. Even since his book was published, Isidore St. Hilaire, who has made hybridism a special study, has published a work in which he asserts, and goes far to prove, that hybrids are sometimes at least just as fertile as their parents. Out of this uncertainty, let us draw one certainty; and it is that nothing is certainly known about it. And also one probability—that offspring may differ from their parents and brethren so very much that there can be no sexual intercourse between them. They may differ less and then there may be intercourse but it will not be productive. They may differ still less, and it may be productive, but the offspring will not reproduce. Still less and they will reproduce, but only for a few steps. Still less, and they will be as fertile as their parents or brethren. Scientific men may give to these degrees of differ-

ence the names of classes, of genera, or species, or what else they will. For here I will venture to remark that much of the criticism and discussion to which Darwin's work has given rise, both in England and in this country, seems to me verbal only. That is, it relates not to the origin and nature of certain existences, but to the language we should employ in speaking of them. What do we gain in real knowledge, when we insist that the word "species" *must* mean this or that, when it *may* mean anything, and very few persons use it in the same sense, or in any definite sense. And as to the question of difference or identity, do we know enough about it to be very positive on any point, except our ignorance? For how many years has the Tertiary formation been arranged into four classes—the Eocene with its one shell in twenty-five now living, the Miocene one in six, the Pleiocene one in two and a half, and the Pleistocene nine out every ten. DesHayes, a great man, has devoted himself to their examination, and has reasserted this with the most emphatic distinctness and the most abundant illustration: and Agassiz now comes and declares it to be all a great mistake. He doubts whether any one shell of the 4 per cent, the 17, the 40, or the 90, has ever been looked upon alive by man.

Far be it from me to undertake to decide between such men. But again let me draw one conclusion, which seems certain; and it is that there is no sure, unerring, and unmistakeable test of specific identity or difference.

If we admit with the qualification and in the way above stated the theory of the production of all things by generative development, and the active operation of this principle of the "struggle for life," and admit also Agassiz's requirement of new creations, and of the orderly succession and coördination of these, we have a theory composed of elements which certainly do not now oppose and destroy each other, but coëxist in harmony, and in mutual support and illustration.

How far shall we carry it? Not to the creation of all things from one beginning, unless farther investigations should remove the immense difficulties which this theory must now encounter, and sustain its probability. But let not the investigation be clouded, obstructed and defeated by the assertion that any theory which calls into being all existing and extinct organisms by some method of successive generative development, cannot be true, and must needs be false and dangerous.

The great difficulty to most minds would be, after all, that which relates to man himself. Man, from a monad! Yet let it not be forgotten, that this is the natural history of every man that has ever been born of woman. At first a nucleated cell, (call it a monad if you like,) not distinguishable from other nucleated cells, which, by segmentation, gives rise to that germinal membrane, from the outer portion of which are formed the or-

gans of animal life, and from the inner those of organic (or vegetative) life; and then, in its uterine development, exhibiting successively resemblances, more or less close, to the lower animals; the human embryo, for example, having, about the twenty-fifth day, the branchial openings and elongated body of a fish, at a later period the imperfect limbs of a seal, and still later the bent limbs of a quadruped. These, and many analogous particulars in the history of the human embryo, make this one of the most inexplicable and yet suggestive wonders of existence. One might well imagine that the "monad" retraces his footsteps along that immeasurable pathway from primeval being, and as it repeats, records them.

While all this is nothing like *proof* that man is also a product of this law of generative devlopment through variation, it may have some tendency to lead the mind in that direction. And how much there is elsewhere in the metamorphoses of nature, to exert upon the mind a similar influence. Tell one who is eating a ripe peach, and after enjoying all the pulp, breaks his teeth against the stone, and being curiously inclined opens that, and finds the solid meat, and opens that again, and puts the infolded plumule under a lens, and sees there the promise of a future tree,—tell him that skin, and stone, and seed and plumule, all are but *changed peach leaves*, will he not be at least as much surprised as if you carried him to a menagerie, and pointing to a hyena, said to him, there stands the father of the "yaller dog" of New England? *

But this notion of man being born from an animal stands in

* I allude, of course, to the January number of the Atlantic Monthly, wherein this strange animal is presented with that wonderful power of word-painting, which is a true daguerreotyping by the sunlight of genius.

But I write this note rather to refer to an article in the North American Review for July, 1857, in which Dr. Holmes, before the controversy about "Darwinism" began, treats many of the topics to which it has given rise, and exhibits his own views of an ever *immanent* God.

No one can admit more cordially than I do, the principle which has been recently so much considered, that God must have had at and from the very beginning of his action, laws, to which he and his universe have always, and, I am willing to say, necessarily conformed. So too, I admit, as cordially, that other principle, that all science, philosophy and reason, lead concurrently to the conclusion, that the "Causa causans" must be always and incessantly a present cause, as *present* at one period of duration as at another, and always directly and universally operative. But why regard these principles as antagonistic? To me they seem not only harmonious, but complementary, and necessary each to the other. If I believe that God is ever present, active and operative, it is because I believe that the laws of order which arise from his own divine nature, permit and require this. If I believe that these laws exist, that he has ever conformed to them and must ever do so, it is because I believe that they are the eternal instruments of his ever active love and wisdom. In the words in which Dr. Holmes sums up the whole matter at the close of his article in the N. A. Review, "whatever part may be assigned to the physical forces in the production and phenomena of life, all being is not the less one perpetual miracle, in which the Infinite Creator, acting through what we often call secondary causes, is himself the moving principle of the universe he first framed and never ceases to sustain."

the way of positive revelation! In my own mind it does not. I look upon the Bible as the word of God: but I do not believe that the first chapters of Genesis teach or were ever intended to teach natural scientific truth; nor does this denial lessen my reverence for what I consider as the moral, spiritual, and religious truth which I believe they do teach directly, or under the form of parable and symbol. And upon the question of the original and physical creation of man, I think that we know no more and no less, and are at equal liberty to think, to argue, and to conclude, as if these chapters had never been written. To me, they do not say one word about it.

But does not this notion stand in utter opposition to all religious belief? Again, I can only say that in my own mind it does not. I believe, most unreservedly and undoubtingly, that man is superior, not in kind only but in degree, to all animals, and is immortal, which they are not. But this belief would not be either shaken or troubled, if science should, upon evidence discovered hereafter, teach, that the Gorilla, which Owen says is most like to man, or the Chimpanzee, which Professor Wyman, with better reason, places higher,—if either or both had given birth, when the fit time had come, to a babe, whose brain and nervous system, with all the residue of its frame, were so organized that the breath of life, of spiritual and immortal life, could be breathed into him, and bear with it all the attributes of human nature,—all those attributes which divide, as by an unfathomable abyss, the man from the beasts that perish, and lift him infinitely above them. At present, science possesses not only no facts which would lead to this as a certain conclusion, but none which would declare it to be a probability. But neither has it sufficient reason for asserting it to be an impossibility. Nor, does it seem to me, that religion would receive a blow, if science should be led by additional discovery and more thorough investigation, to go not only thus far, but so much farther, as to account for the various kinds of men by asserting that the brown oran-outang that lives among the brown Malays was their progenitor; the black gorilla the father of the black races, among which he is still found; other simiæ the parents of other human families; and some one fairer than the rest, the remote ancestor of the Circassians, whose superiority over their progenitors was so great that they had rooted him out from the earth!

But let us consider the general relation of this hypothesis to religion. I am perfectly willing to confess that the theory propounded by Darwin, as it rests upon excessively minute changes, and those produced by what he calls "accident," (of which word, however, and of his use of it, he offers much explanation) seemed to me to have a tendency to obscure the thought of providential causation and government; and that I was first led to reason out, as well as I could, the probability and effect of more salient changes in the offspring, by its appearing to open the door

to this thought somewhat more widely. But aside from this, and indeed from any reference to this or any special question or theory, may it not be well to remember, that natural science belongs, mainly at least, to the intelligence of man, and to his outer life, while religion belongs, mainly again, to his affections, his motives and his inner life. Hence, entirely different faculties and functions of our common nature are brought into exercise in reference to science, from those which are invoked by religion. It is a good and wholesome thing for a man to become religious because he chooses to be so, and loves to be so; and it is good for him to compel himself to make this choice. He cannot indeed become religious on any other ground or in any other way. And therefore Divine Providence has mercifully guarded him, not only from the external compulsion, which, as all men see, cannot reach the heart, but from the compulsion of his own intelligence, which might be equally injurious.

In investigating the claims of science, he must call upon his intellect to look sharply at the facts, the logic, the arguments and the conclusions; and this is all, or nearly all. But he must choose and hold his faith, not by means which logic disdains and denies, but by asking of logic to do all that it can do and and the best that it can do, as the instrument of something higher than itself, which can take up and complete the work which mere logic must leave unfinished.

How easily could God have written his word and his truth in fire upon the sky, and in gold upon every leaf or stone, if all he had desired was the intellectual advancement of man. We may infer from his course of providence, that he desires this, only as a means to an end; and as an instrument of that moral and affectional improvement, which must be man's own coöperative work. Therefore it is, that religion never has been, and I think never will be fortified by the demonstrations which belong to ascertained science; and hence it is also that no science, and no mere truth has ever yet been suffered to arise on the world, and none I think ever will be, that does not leave man free to be irreligious if he will; although all true science offers him much to feed upon and to rejoice in, if he loves to look upon the truth he learns as aliment for his religion.

To every creature is given a tendency and a capacity to seek and find and appropriate that food which agrees with its own nature. When a willow tree sends a root far in one direction to a ditch where it may drink its fill, and a neighboring grape vine sends its root as far in an opposite direction and finds a heap of buried bones, we have but the operation of the same law, by virtue of which if ten men read a book, it may be to them ten books; for each will read the same words, and then translate them in his own way. It is an old saying, that what one brings home from foreign travel, depends upon what he car-

ries with him. So it is in the journeyings of the mind. Let that go where it will it carries itself, and uses itself as the organ for giving form and effect to all that it receives.

The poet may say that the undevout astronomer is mad; but astronomy, and every science cultivated among men, has those who are devoted to it with the most faithful assiduity, and who extend its borders and enlighten its dark places, and who are, nevertheless, utter unbelievers as to God and religion; and find in their science support for their unbelief. To minister to religion is the highest, the consummating work of science; but science cannot render this service where there is no religion to accept it. So will it be with the theory of the creation of all things by successive generative and variant production, if it be established in any form whatever.

This man will read it to whom the idea of God is an offense and a pain. His unbelief holds him in subjection; and when he reads any book, or studies any subject, he reads with clouded eye and mind all that favors religious truth, but brightens at once when he gets a fact or an argument for his unbelief, and dwells on that as a choice morsel. He will study this new theory, and find in it new evidence that God is a mere superfluity; and he will say exultingly, now we have proof that the laws of the world and their own necessity are all that a truly rational mind can ask. And he will deny, or forget, that there is no possible conception which so imperatively demands a lawgiver, as law; and none which so requires a cause to set it in action, as an active necessity.

Another man who loves to believe that God forms and fills and *is* the universe, and that there is no other God, will find here abundant support for his opinion, and will rejoice in the evidence this theory affords of the universality of law and the connection of all things by gradation into unity. And he will forget, or will not know, that all this implies design, and purpose, and will, and therefore personality.

And a third man will see in this theory new proof of the eternal working of the personal God in whom he believes. He will rejoice at the evidence it offers that God loves to bless every entity of his creation by using it as his own instrument and as the means for farther creation; that preservation is continual creation; and that he forever puts forth the same power, born of the same love and guided by the same wisdom, that in the beginning laid the foundation of the universe deep in that infinite which no plummet of human imagination ever can sound. To such a mind it will be a new proof, that from God's own nature, there came forth laws of order, in which, through which, and by which, he has ever worked, from a beginning, which when we try to think of it, recedes faster than thought can follow.

Cambridge, May, 1860.

ART. II.—*Notes on the Habits of the Common Cane,* (Arundinaria macrosperma, *Michx.*); by HUGH M. NEISLER, Corresponding Member of the Essex Institute, Salem, Mass.

THE common cane, as it springs from the seed, bears no slight resemblance to some of our coarser and more reedy Panicums; rising, during the season of its growth, according to the more or less favorable circumstances of its locality, from the height of a few inches to that of several feet, forming a straight unbranched culm, with a bud at every node. The culm itself completes its entire growth the first season, and though it endure for years, even to the time of its flowering and fruiting, never afterwards increases in height, only becoming tougher and stronger by the gradual deposition of siliceous matter on its surface, and ligneous matter in its substance.

The second year, circumstances being favorable, its lateral buds are developed—forming slender, upright branches—whose growth in length likewise terminates with the season, and which rarely grow much higher than the summit of the culm.

The third year, the buds of these lateral branches are developed, and they, in their turn, form other lateral branches with their buds, and thus the plant continues to grow until it flowers and fruits, until its existence is terminated by accident, or it becomes so much crowded that there is no longer room for its growth in this manner. However, the buds of one season do not invariably put forth the next; but, if circumstances are unfavorable to their development, they may remain dormant for a time, and then put forth or eventually perish.

The first year, besides the growth already described, the plant throws off one or more subterranean culms, popularly termed "chain roots," differing from the others only in being white, with very short internodes, and these clothed with imperfect sheaths, or naked, the sheaths becoming obsolete or, as is more commonly the case, reduced to a circle of rootlets around the node. From the buds of these subterranean culms springs the second year's growth of cane. This is subject to the same laws and grows in the same manner as that of the first year, likewise throwing off its subterranean culms, from the buds of which springs the cane of the next season, and so on it proceeds for years, new cane coming up each successive season, until it is so crowded there is no room for more to grow. The buds of these subterranean culms may, like the others, be developed the year after they are formed, or remain dormant until circumstances favor their growth.

The cane of each succeeding season becomes stouter and taller than that of the preceding; and as the cane of one year is overtopped and shaded by that which grows up the next, it dies out gradually and gives place to the larger cane of some future season; hence arises the great uniformity in size, observable generally in the plants of a cane-brake.

It may not be amiss, in this place, to observe, that the cane is described by authors as "branching towards the summit"; this is correct as applied to plants growing in a crowded cane-brake, where the development of the lateral buds is prevented; but where it is uncrowded and free to grow, it presents itself, clothed from the ground upwards, with numerous erect, closely appressed branches, as the seedling plant is described near the beginning of this article.

After the growth of a number of years, and when the cane has reached as great a size as the circumstances of the locality will admit of, it springs up only in such places as may yet be unoccupied, until the whole brake becomes as thickly crowded as possible. After arriving at this point, its growth altogether ceases, excepting from the few buds that may be formed at the summit, and may chance to be freely exposed to the light and air. In this condition it will remain for a series of years, until the time of flowering arrives. If, however, whilst in this condition, any or all of it by chance be destroyed during the winter, its place will be supplied by cane of equal size the following season, from the buds of the subterranean culms; but if an accident of this kind happens during the growing season, it never grows up again, the whole plant dying; though if any escape the accident, the extension of their "chain roots" may gradually fill up the vacancies left by the plants that have perished.

The size which the cane ultimately attains is as various as the soil in which it grows. In places peculiarly unfavorable, it may not be higher than six inches; in the "Piney-woods" swamps, in my immediate vicinity, ten or twelve feet is about the average height. On Flint river it is common to find it as high as twenty-five and thirty feet; whilst I have seen occasional specimens cut from the swamps of the Uchee in Alabama, which, though not actually measured, I should judge, could not have fallen far short of forty feet. In the same cane-brake, too, if covering ground which presents striking diversities of soil, it will be found to vary greatly in size.

We have above remarked that when cane reaches its full size, it remains almost unchanged until the time of flowering: how long this may be I have not the means of determining. It is evidently many years. A piece of land, many acres in extent, was once pointed out to me in the low grounds of Flint river, by a gentleman residing in the vicinity, who informed me that on his removal to that country, it was covered by a dense growth of large and full-grown cane, and that it flowered and fruited fifteen years thereafter. If in this instance we suppose it to have required ten years to bring it to the condition in which he first observed it, its time of flowering would be brought to twenty-five years—the age at which, in this region, it is pop-

ularly supposed to flower and fruit. This, however, is but a supposition.

When the flowering once begins, it takes place in every plant in the cane-brake at the same time, whatever may be its age, its puberty depending not on the actual age of the cane itself, but upon the time that has elapsed since the germination of the seed from which it has either directly or indirectly sprung. When the time comes, it flowers, whether it has grown up from the ground ten or twenty or more years before, or whether it has yet to complete the first season's growth. In the spring of 1857 I found a small field from which the cane had been cut the preceding winter. The husbandman had been so careless in clearing out the young cane that had come up amongst his corn, that great numbers of them were left growing about. They were generally about three-fourths of an inch in diameter, but only some five feet high, and still so tender and soft that they could be easily crushed between the finger and thumb to within one and two joints of the ground; yet every plant was in full bloom. There the time of flowering had overtaken it when little more than two months old. It is, however, no uncommon thing to find individual plants in bloom, sometimes several years before the general flowering commences. This is, however, only an exception, an occasional precocity brought about by some extraordinary cause.

With the ripening of the seed ends the life of the plant. It then perishes root and branch, as entirely as an annual grass; and if the same ground be ever occupied by cane again, it must be produced anew from seed. Authors, I judge, have been unaware of this fact, from their describing the plant as fruiting at stated periods, or at more or less distant intervals; and this error perhaps may have originated from their noting the times of the flowering of the plant, as it occurred in different localities; or, if observed in the same place, the intervals have been so long between the times of flowering, that the fact of its having been in the meantime reproduced from seed, may have escaped recollection.

The question here presents itself, Whether there are two species or varieties, or but one? Within the comparatively narrow limits to which my own researches have been confined, I have had opportunities of gathering the flowers of both large and small canes; but specimens have yet to be collected that present a difference. As to the question in general, if the reader bears in mind, that the cane in the earlier years after the germination of the seed is much smaller than that which follows at a more distant day, he will readily see, that if both should chance to survive for years, they might appear so different as to be regarded as distinct varieties, if not species, especially as the flowers are rarely procurable to prove their identity. The same may be said of the differences produced by differences in the character of the soil in which they grow.

Taylor Co., Geo., 1860.

ART. III.—*Experiments on the forms of Elongated Projectiles;* by OGDEN N. ROOD, Prof. of Chemistry in the Troy University.

ALTHOUGH during the last few years great attention has been paid in Europe to the improvement of rifle projectiles, yet much obscurity seems still to prevail on this subject, as is sufficiently evinced, by the different and opposite courses pursued by the several governments; the variety in the shapes of the balls employed at the present time being almost as great as that in the patterns furnished by a kaleidoscope.

The inquirer in this department meets with very *general* answers, and is too often told in substance, that the accuracy of performance is directly proportional to the excellence of the ball's model; it being in the meanwhile by no means particularly apparent in what such excellence consists.

An explanation is found in the fact, that there is perhaps no other field of investigation, in which so great a number of experiments is essential to the establishment of single and even isolated facts—wind and weather, slight changes of temperature in the tools employed, or in the form and fit of the projectiles, as well as other and more obscure causes, all largely influencing the results, contribute and combine vastly to complicate what at first glance might seem a moderately simple problem.

A series of experiments lately instituted by me, had for their object the examination of a few of the more obvious considerations relative to the accurate flight of elongated projectiles, such as length, the form of the base, and other points.

The rifles employed, were, with the exception of No. 5, made by Nelson Lewis, of Troy, N. Y., and were of the model sometimes called "Kentucky," or more properly, "Improved American."*

Our marksmen know the quality of Mr. Lewis's work, although it may be well to state, once for all, that rifles of this model as manufactured by him, and a few other makers in the United States, hold much the same relation to the English or Continental arms that the Oertling balance does to the scales of the apothecary; a fact which has not escaped the notice of the author of the article on Gun Making, in the Encyclopædia Britannica, 1856, vol. viii, p. 101, where, after a description of this rifle as made by James, of Utica, N. Y., and an account of its performance at 220 yds., he concludes: "The whole of the ten shots would have gone into a small sized playing-card. A feat of this kind is probably unparalleled in Great Britain, and it may draw

* For some account of these rifles see an excellent article published in the Atlantic Monthly for October, 1859.

the attention of our own makers to the propriety of diminishing the calibre, and increasing the speed of the ball."

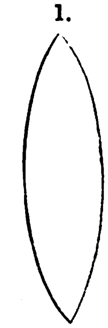
Dimensions of the rifles employed.

	Length of Barrel.	Weight. lbs.	Weight. oz.	Calibre.	Twist of grooves gaining, ending in one turn in inches.
No. 1.	30·5 in.	16	0	0·46 in.	
No. 2.	31·0 "	9	0	0·34 "	42·35
No. 3.	30·4 "	7	4	0·42 "	
No. 4.	12·0 "	2	12	0·36 "	47·37
No. 5.	5·5 "	0	8	0·20 "	12·85

They were all provided with the sights known as the "Globe and Bead," as well as with set locks. Nos. 1 and 2 were supplied with the false or loading muzzle, and all with the exception of No. 5 had "guide-starters," which ensured the accurate placing of the ball in the barrel. Linen patches were in every case employed, not greased, but moistened, it being now pretty well understood that the latter afford the more accurate practice. As a general thing, after each shot the barrel was wiped out with a slightly moistened rag, dampness being guarded against by a repetition of the process with one that was dry.

Length of the Projectile.—As has been intimated, great variety of opinion seems to exist as to the proportion which the length of the ball should bear to its diameter measured at the base, balls being used in Europe whose length ranges from 1·36 to 3 diameters. The advantages of length are generally known and there are writers who advocate progress in this direction, imagining naturally enough that a ball modeled after the lines of a ship would experience less resistance in passing through the air than its shorter and blunter rival. Some months ago, filled with this idea, I constructed a ball of the model shown in Fig. 1, sharp both at bow and stern, and having of course its centre of gravity farther forward than is usually the case with solid projectiles. Its penetrating power at short distances was great, but its complete and total failure otherwise, resulting from the impossibility of communicating a sufficiently high rotary motion, led to an examination of the rates at which it is necessary for balls of different lengths to revolve in order to secure accuracy of flight.

1.



Diam. ·36 in. Length 1·27. 3·53 Diameters long. Weight 222 grs.

Now it is evident that if the velocity with which any ball leaves the rifle-barrel be known, and likewise the twist of the grooves, the rate of revolution of such ball per second is easily found; thus, if the initial velocity should be 1000 feet per second and the twist one

turn in 5 feet, it is plain that at the moment of leaving the muzzle, the ball will be revolving at the rate of 200 times per second.

To ascertain then the initial velocity, I constructed two ballistic pendulums, similar to those used by Robins. The total weight of the larger pendulum was 21·75 lb., and it happened, by chance, that 120 of its small swings were performed in two minutes; consequently its centre of oscillation was distant 39·13 inches from its axis of suspension: the centre of the pendulum was 39·7 inches from the axis, while the centre of gravity was 37·4 inches distant from the same point: therefore we have,

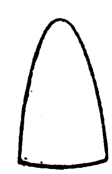
$$\left.\begin{matrix} 39{\cdot}7 : 39{\cdot}139 \\ 39{\cdot}7 : 37{\cdot}4 \end{matrix}\right\} :: 21{\cdot}75 : 20{\cdot}2 \text{ lb.}$$

or the pendulum resisted the impact of a body in motion as though its weight had been only 20·2 lb. The axis was made of a bar of steel, two sides of which were ground to an edge. Instead of the ribbon used by Robins to measure the chords, a narrow strip of paper was employed, which was fastened to one side of the pendulum on a line with its centre, that is 39·7 inches from the axis.

The smaller pendulum was similar in construction: its weight was 7833 grs., it made 93 oscillations in one minute, consequently its centre of oscillation was 16·29 inches distant from the axis; the centre of the pendulum and its centre of gravity were distant from the axis 16·6 and 15·2 inches; it resisted therefore as though its weight had been 7038·4 grains.

For barrel No. 4 I succeeded in constructing, after many trials a ball or picket, on the American model, Fig. 2, whose flight was accurate up to 500 yards; its dimensions are given below. This ball was fired with different initial velocities at targets of *pasteboard*, placed at certain distances from the rifle; an examination of the holes made, at once indicated whether the rate of rotation sufficed to compel it to fly truly and point foremost; for if this were not the case, the picket of course, made *oval* holes or struck the target sideways.

2.



Ball No. 4.
1·67 Diam. long.

Ball No. 4.

Length ·62 in. Diameter ·37 in. Length in diameters, 1·67. Weight 113·34 grains.

Initial velocity.	No. revolutions per sec.	Distance.	Point foremost.	Oval.	Sidewise.
520 ft.	131·7	20 ft.	1	4	6
520 "		36 "		2	2
691 "		20 "	3	1	1
691 "	175	36 "		2	2
691 "		165 "			8
849 "	215	75 "	1	1	2
849 "		165 "	2	1	2
965 "	244	165 "	8	6	3
1128 "	285·8	1500 "	Flight accurate.		

It will be seen from these experiments that a ball of the above model, and 1·67 diameters in length, must make 280 revolutions per second to ensure its accurate flight, when discharged with an initial velocity of 1100 feet per second; what rate of revolution would be necessary to sustain it point foremost, were the initial velocity different, is another question, and one which will presently be noticed.

Let us examine the case of a ball slightly *longer* than the above. Rifle No. 2 was now employed, its ball was slightly blunter than No. 4; it had often made excellent practice at 220 and 500 yards.

Ball No. 2.

1·77 diameters in length. Weight 105·01 grains.

Initial Velocity.	No. Rev.	Distance.	Point foremost.	Sidewise
1051	298	165 ft.		all
1199	340	660 "	all	

By a comparison with the last table, it will be seen that this slight increase of the length has carried with it the necessity of 50 additional revolutions per second. The lowest initial velocity I have ever actually employed in this rifle for target practice was 1774 feet, the highest 1917 feet, so that the number of revolutions made by the ball in actual practice was either 503 or 543.

Balls 2 and 2·5 diameters in length.

A longer ball for barrel No. 4 was now constructed: its weight was 141·5 grains, diameter ·36 in., length ·74, so that it was in fact 2·05 diameters in length. The following results were then obtained:

Ball 2·05 diameters long.

Initial Velocity.	No. of Rev.	Distance.	
1063	269	18 ft. 42 ft.	all sidewise.

The above shows that with this model 269 revolutions are altogether insufficient, if the initial velocity be as high as 1063 ft.

The length of the ball used in the Swiss federal rifle is 1·0039 inches, its diameter 0·41 inches, or it is 2·44 diameters in length, weight 257 grains, weight of the charge 62 grains. Making use of the British empirical formula $V = 1600 \sqrt{\frac{ap}{w}}$, we have the initial velocity $= 1600\sqrt{\frac{3\ 62}{257}} = 1361$ ft., and as the twist is one turn in three feet the ball leaves the barrel making 453 revolutions per second.

The initial velocity in Jacobs' rifle, judging from his statements, must be about 986 feet: as the twist is one turn in 28 inches, the ball makes 422 revolutions per second.

Now why not, it is often asked, project either of these balls with an initial velocity of 1600 or 1700 feet per second? Simply because the above rates of revolution then become wholly insufficient. To test this point, I constructed for barrel No. 2 a ball weighing 136 grains and only 2·08 diameters in length, consequently not requiring as rapid a rotary motion as the Swiss ball.

Ball 2·08 diameters in length.

Initial Velocity.	No. of Rev.	Distance.	
1715 ft.	486	165 ft.	All sidewise.

It would probably require a rate of at least 600 revolutions per second, and the Swiss ball being again much longer would hardly fail to need a rate of from 700 to 800.

The twist of Whitworth's rifle (ball 3 diameters long) makes one turn in 20 inches: its initial velocity is not given, though it is understood to be greater than that of the Enfield: *if* it be as high as 1600 feet, the rate of revolution of the projectile will be 960 times in a second; if 1700 feet, 1020!

The above mentioned experiment will perhaps suffice to establish the fact that for any given projectile, the necessary rate of revolution increases rapidly with the augmentation of the initial velocity.

The inability to use larger charges of powder prevented me from pursuing the investigation farther in this direction, but it still remained possible to invert the order of proceeding and to examine whether at very *low* velocities the necessary rates of revolution for projectiles 2 and 2½ diameters long rapidly decreased. An elongated ball for barrel No. 5 was now constructed: its length was ·38 inches, diameter ·2, *i. e.* 1·9 diameters in length; weight 20·8 grains. The initial velocity was determined with the smaller pendulum and found to be 232·6 feet: the velocity was determined also from the time of its flight: a distance of 15·41 feet gave it 233: the average of 14 shots at 44·2 feet made it 216.

Ball 1·9 diameters in length, weight 20·8 grains.

Initial velocity.	No. of Rev.	Distance.	
232	216	75 ft.	Flight accurate.

Another ball was now constructed for this barrel; the results were as follows:

Ball 2·3 diameters in length, weight 26·5 grains.

Initial velocity.	No. of Rev.	Distance.	
202	188	75 ft.	Flight accurate.

A third ball, 3 diameters in length, was finally made for the same

A third ball, 3 diameters in length, was finally made for the same barrel: it was cast with inclined bands on it, extending for two-thirds of its length and corresponding to the grooves of the barrel:

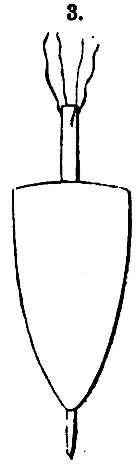
Ball 3 diameters in length, weight 42·3 grains.

Initial velocity.	No. of Rev.	Distance.	
157·9 ft.	147·5	30 ft.	Flight accurate.

Patches were not used with these three balls.

As a termination to this portion of these experiments, I constructed the model seen in Fig. 3, 2 diameters in length and provided with a steel axis: it was caused to rotate by the thread on which it was suspended: small discs of paper divided into green and red sectors were attached, it having been previously ascertained by a revolving machine, with what rate of revolution each disc assumed a neutral tint. The model being made to rotate was allowed to fall from a height and it was found that six revolutions per second enabled it to remain point foremost in a fall of five feet, while 20 per second sufficed for a fall of 45 feet.

3.



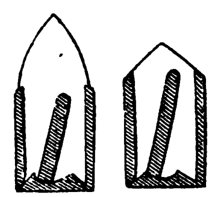
The only question that now remains is, if 700 or 800 revolutions per second are necessary for the flight of a projectile $2\frac{1}{2}$ diameters in length at a high velocity, why should this rate of rotation not be communicated to it? The answer is found in the practical difficulties; the friction and recoil become enormous, the act of discharge twists the rifle over sidewise and out of the line of sight. In the American Rifle, with a gaining twist, where the friction, twisting &c., are reduced to a minimum, it has been found that the maximum number of revolutions per second that can properly be communicated to a ball is from 500 to 550, and many will be ready to deny that even this rate can be employed with other than very heavy rifles of very small calibre without greatly impairing the accuracy. According to Chapman, a rifle having a regular twist of only one turn in four feet, calibre 80 round balls to the pound, charge of powder 2 inches to the bore, [initial velocity about 1680 feet, rate of rotation 420 times in a second] will, "when fired, twist over sidewise in spite of all you can do, and also kick or recoil very severely.* Surely if such recoil and twisting can be felt and seen, the tendency of the bullets to scatter and strike the target in a circle and not in a straight line is easily accounted for."

* It appears that European projectiles moving with a low velocity are subject in the longer ranges to a horizontal drift independent of the wind. This is attributed to the action of the grooves in the rifle, and has been found to correspond with their direction. No such deviation was observed by Gen. Jacobs (page 27 of Rifle Practice,) and he attributes it to the heavy recoil of the rifles used. As far as I know, it has never been observed in the American rifle where the recoil is very small, and the *trajectory very much flattened*, though it is the custom of our marksmen on calm days to practise at targets placed indifferently at 110 and 500 yards.

But where accuracy is aimed at, it is highly desirable not to employ a rate of revolution such as is able barely to cause the projectile to fly point foremost. A very notable excess above this quantity is essential for good practice.

One point remains to be noticed, viz.: the disfiguration which balls longer than 1·9 diameters suffer, when discharged with more than a very moderate velocity, in their passage through the necessarily strongly inclined grooves of the rifle. Gen. Jacobs states (page 23 Rifle Practice) that his projectile 2½ diameters in length performed admirably with a charge equal to one-fifteenth of the weight of the ball, but adds that he found, "A very slight increase of the charge caused the lead to change its shape under the pressure of the gunpowder so that a ball of the shape of Fig. 4 came out like Fig. 5. The lead was found totally incapable of preserving any resemblance to its original form under the pressure of a charge of powder equal to one-eighth the weight of the ball." From this dilemma the General escaped by making the forward part of the ball out of zinc; Whitworth provides for this difficulty by the use of pewter, which again necessitates a polygonal bore and a regular twist.

4 and 5.



Finally, during the last few years many of our best gunsmiths and marksmen have endeavored to increase slightly the length of the balls of their rifles above the standard dimensions given below, but in spite of the heavy charges used (3½ inches to the bore sometimes,) it does not appear that their efforts have been crowned with success, every fifth or sixth shot straying off some inches, or even, in the worst cases, striking the target sidewise at 220 yards.

It is perhaps hardly necessary to dwell farther on this matter of length, for while all the foregoing tends to explain why it has been found necessary in Europe to use low initial velocities with these *highly* elongated projectiles, it at the same time gives us little reason to expect that they will ever be able to compete with their shorter and more manageable rivals.

Below are the dimensions and weights of a number of balls belonging to first-class American rifles, each of which has been distinguished for its accurate practice:

Diameter of the Base.	Length.	Length in Diameters.	Weight in Grains
0·36 in.	0·62 in.	1·72	112·7
0·35 "	0·62 "	1·77	105·
0·46 "	0·82 "	1·78	243·6
0·47 "	0·87 "	1·83	249·
0·45 "	0·86 "	1·91	265·8
0·44 "	0·85 "	1·93	225·

But assuming the length of the projectile to be within the above standard American limits, it is still possible to increase its weight by adding matter about the point, which of course becomes blunter. A ball was constructed of the same length as No. 4, but weighing 121·1 instead of 113·6 grains, the $7\frac{8}{10}$ additional grains of lead being disposed about its point. As has before been seen, the old ball with an initial velocity of 1128 feet, and with 286 revolutions, had been accurate in its flight, but with the same charge of powder the new ball flew sidewise: the charge was increased; still with an initial velocity of 1443 feet, and making 365·5 revolutions per second, the new ball flew untruly, making oval holes in the target at 165 feet.

At present it seems to be the opinion of our best marksmen that a projectile constructed with a very moderately heavy point affords proportionately better practice at 500 than at 220 yards.

The Curve of the Projectile.—The bounding curve of the projectile has been largely experimented on in the United States, and has been slowly perfected by a class of men whose stock of mathematical knowledge was, and is, exceedingly limited: the results of their experimental labors are on this very account the more interesting.

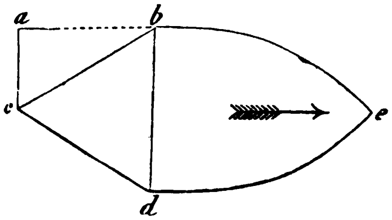
I selected balls belonging to three excellent and well-tried rifles (by different makers) and placed them under the compound microscope, which was arranged so as to magnify only about five diameters: a camera lucida being attached, by its help the magnified images were transferred to paper. The curves were examined and found to be portions of ellipses, the main difference being in the points of the balls, which were slightly sharper. In two of the balls, with this exception, the coincidence was almost perfect: the curve of the first formed a portion of an ellipse whose diameters were in the proportion of 5 to 0·9; in the second the diameters were in the proportion of 5 to 1, in the third of 5 to 0·86. The agreement was pretty close with the third ball, but its bearings had been made more nearly parallel with its axis than was the case in the others, consequently here it differed slightly from an ellipse.

Form of the base of the Projectile.—The base of most balls now used is either flat, slightly convex as in the American model, or more or less deeply concave as in all those constructed on the expansion principle. It is generally admitted that balls having a flat base and moving with a velocity greater than 1200 feet per second, leave behind them a perfect vacuum, which, in addition to other sources of resistance, retards their progress by a pressure in front of 15 pounds to the square inch.

As the air is assumed to rush into a vacuum at the rate of 1150 feet per second, it now becomes a question whether the

base of the ball could be so shaped that in its passage through the air this vacuum might in great part be prevented by air rushing in at a certain, but lower rate than 1150 feet. It is evident that the lower this rate is made, the more do we subtract from the pressure in front, that is, diminish the resistance to the flight.

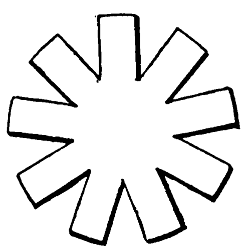
6.



$bd = cd$
$bd \times 1.9 = ce$

Suppose the base constructed like *bcd*, Fig. 6, and the velocity of translation 1100 feet per second, then it is evident that while *ab* is the measure of the velocity with which the point *b* moves from *a* to *b*, viz: 1100 feet, *ac* will be the measure of the velocity with which the air must rush in to prevent a vacuum, and by construction *ac* is only 635 feet. We should be led to expect that a projectile of this shape would experience less resistance than one provided with a flat base, until the velocity of translation was increased so that *ac* became equal to 1150 feet, which in the model used by me takes place of course when *ab* or the velocity of translation is 1992 feet per second. Every effort was made to give these double cones as great a length as possible; many models were tried, and the length was slowly diminished until the new projectiles flew truly; finally, moulds for two balls of this model were perfected for rifles No. 2 and No. 4, and after some slight alterations the new double cones rivaled in accuracy of performance the pickets with flat bases. To accomplish this, it was found necessary to cut the patch as seen in Fig. 7, which ensured accurate loading.

7.



Patch for double cones.

Below are the results of a number of experiments on the time of flight of the old flat-ended balls and of the new double cones, together with their average velocities from 110 to 500 yards.

Single Cones. (Weight 113·34 grains.)

Rifle No. 4. Initial Velocity 1128 ft.

Distance.	Time of Flight.	Average Velocity.
110 yards,	·3323 sec.	993 feet.
220 "	·6947 "	950 "
500 "	1·885 "	795 "

Double Cones. (Weight 109 grains.)

Rifle No. 4. Initial Velocity 1138 ft.

Distance.	Time of Flight.	Average Velocity.
110 yards,	·32795 sec.	1006 feet.
220 "	·71725 "	920 "
500 "	2·05680 "	729 "

Single Cones. (Weight 105·01 grains.)

Rifle No. 2. Initial Velocity 1917 ft.

Distance.	Time of Flight.	Average Velocity.
220 yards,	·4175	1580 feet.
500 "	1·3080	1146 "

Double Cones. (Weight 100·8 grains.)

Rifle No. 2. Initial Velocity 1867 ft.

Distance.	Time of Flight.	Average Velocity.
220 yards,	·4552	1449 feet.
500 "	1·4120	1062 "

It will be seen from these tables that the flight of the double is *slower* than that of the single cone, particularly at high velocities: this disproportion diminishes as we use lower velocities, but even here the new seems to possess no advantage over the old form—the sharpness of the stern not at all compensating for the necessary bluntness of the bows. It occured to me that this inferiority in the flight of the new balls might be owing to a disfiguration in their shape produced by the explosion of the powder: a number of them were accordingly fired into a bank of snow distant 500 yards: when dug out they were found to be as perfect in their proportions as at the time of loading.

It would seem that if the conical has any advantage over the flat base, it is only at velocities as low as 400 or 500 feet per second. These experiments also tend to show that the vacuum behind a projectile does not suddenly cease at a velocity of 1150 feet, but that its diminution is very gradual: they farther indicate to some extent the importance of making the forward part of the ball *sharp* when a high velocity is desired. Indeed, the air struck by the forward part of the ball seems to be thrown from it with such force, at high velocities, that a vacuum is produced behind it, whatever its form may be; and the vacuum is more complete the blunter the point is made. This may account fully for the disadvantage of the double cone.

It is well understood that the *weight* of the projectile exercises much influence on the time of its flight at the longer ranges: the time of flight of a ball weighing 105·01 grains and starting with a velocity 1917 feet has already been given: below are the results obtained with a heavier ball.

Rifle No. 1. *Weight of Ball* 243·6 *grains.*

Initial Velocity 1602 ft.*

Distance.	Time of Flight	Average Velocity.
220 yards.	·477 sec.	1480 ft.
500 "	1·345 "	1115 "

Although the difference in the initial velocities was 315 feet, yet at 220 yards the difference in the average velocities was

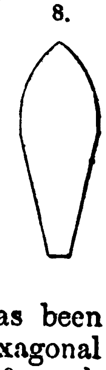
* The initial velocity was *calculated* from determinations made with rifles Nos. 2 and 3. The determination from No. 2 when reduced gave it 1582·5 ft., that from No. 3, 1621·5 ft., the difference being only 39 ft.: the mean of these numbers is given above.

only 100 feet, and at 500 yards it was reduced to 31 feet; showing that after a flight of some 600 yards the lighter projectile is overtaken by the heavier. It may be remarked here that the average velocity of these two rifles during a flight of from 500 to 600 yards, is as high or higher than the *initial* velocities of many European guns! Thus the French Tige Rifle has an initial velocity of 1023 feet, the Enfield Rifle 1115 feet, the Belgian chasseur carbine 1007 feet, United States new rifle musket 963 feet and the altered Harpers Ferry rifle 914 feet per second. Certainly Maj. Barnard is justified in his animadversions on the evils which have attended the introduction of the "shot-gun principle" into modern rifles; and with much reason he exclaims—"a decided step has been made *backwards* in losing that most essential element to range and accuracy, initial velocity."*

Position of the Centre of gravity.—In many European projectiles, every effort is made by hollowing out the base to throw the centre of gravity "well forward," in order that the disposition of the ball to fly point-foremost may be encouraged as much as possible, and for the furtherance of the same desirable end, grooves are usually made about its cylindrical portion That the first proceeding exercises a notable influence (at low velocities,) is generally admitted; that it virtually lowers the specific gravity of the ball and therefore retards the flight is no less certain; and if the rudder-like action of the grooves is admitted, their presence also entails a farther loss of velocity.

I have not as yet found time to experiment with either hollow or grooved balls, but the results obtained with solid projectiles, seem to show that it is of small moment whether *their* centre of gravity be situated a little before the middle of the longer diameter, or a little behind it; thus, in the double cone of which mention has been made the centre of gravity was forward of the middle of the axis, but in spite of this, it was found to require almost (if not quite) as many revolutions per second as ball No. 4. Balls were also constructed like Fig. 8 and fired with an initial velocity of 1682 feet when with a rate of 477 revolutions per second; they struck the target sidewise at 165 feet: a velocity of 1060 feet was then tried, when 268 revolutions proved wholly insufficient to project them point-foremost a distance of 12 feet.

8.



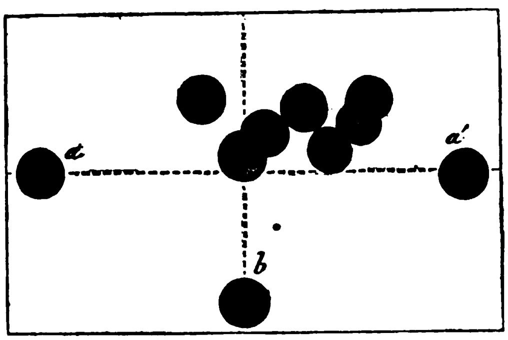
It may not be amiss in closing this article to offer a few remarks on the accuracy of the *American* rifle as compared with those now used in Europe.

In England it is admitted that the best practice has been obtained by Mr. Whitworth of Manchester with his hexagonal projectile 3 diameters in length, in a covered gallery 500 yards

* This Journal, vol. xxix, p. 197.

long. Mr. Whitworth states, or is reported to state, that he succeeds in projecting ball after ball into a circle but little more than two inches in diameter, and that he will not rest satisfied till he has fired a ball from one of his rifles down the barrel of another, placed at that distance! This excessive hopefulness of Mr. Whitworth might cause some to hesitate at the story of the two inch circle; assuming it however to be true, it may possibly be shown that the American rifle under like circumstances will do as well or better.

9.



One-half of full size.

The anexed target was made by Lewis with a rifle similar to No. 1, (provided only with globe and bead sights), at a distance of 220 yards, in the presence of over a hundred persons at the yearly trial of skill held at Waltham, Mass.—of course in the open air. The average distance of the shots from the centre is 1·038 inches. Any marksman upon inspecting it will at once see that shots *a* and *a'* were carried to the right and left merely by the wind, and that in a properly constructed gallery all the shots except *b*, would have been included in a circle one inch in diameter. It will also be acknowledged that in shots fired in a covered gallery the deviation with a good rifle is proportional to the distance traversed, though in the open air it is always somewhat greater owing to the wind producing more effect proportionally on the flight of the ball as its velocity becomes lowered.* Therefore as 220 yards : 500 yards :: 1 in. : 2·27, or nine out of the ten shots, at 500 yards, would have been in a circle 2·27 inches in diameter—practice as good as reported by Whitworth.

But what was the nature of the manipulation in each case? In Mr. Whitworth's by the help of "certain appliances with reference to the recoil, guarantees were obtained that each shot should be taken under similar circumstances. The gun was fitted accurately into a frame resting upon a perfectly level plane, and the recoil was compelled to take place in a line precisely

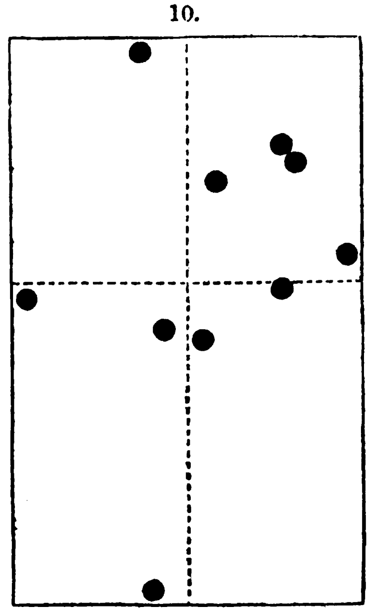
* The anterior probability of this statement may perhaps be rendered stronger by a comparison of the actual with this theoretic deviation. Rifle No. 1 when fired in the open air by only moderately skilled marksmen, gave—

Distance.	Theoretical Deviation.		Actual Deviation.
110 yd.	....	....	0·7 in.
220 "	1·4	+0·2	1·6 "
500 "	3·69	+3·02	6·71 "

parallel to its axis and could be calculated to a nicety. Again, when the object was to prevent any recoil, there was no difficulty in doing so."* All of the above mentioned precautions being no doubt essential to counteract the evil effects of the heavy recoil and of the *twisting* of the piece. With the American rifle no such artificial bolstering was employed, the barrel was rested on a common shooting bench and the stock held honestly to the shoulder of the marksman, the friction and recoil being so insignificant as not to require other contrivance.

But the Whitworth-gun was tried in the open air at Hythe, April, 1857, in competition with the Enfield rifle, to which it appears to have been greatly superior. We are not informed whether "the firing machine" was transported and used on this occasion; be that as it may, the statement made as to the results obtained is, "that when both had a range of 500 yards the superiority of the Whitworth was in the proportion of three to one." As the absolute deviation of the Enfield rifle at that distance is 28 inches, the deviation of the Whitworth rifle must have been about 9·3 inches, or the shots averaged that distance from the centre of the target. Chapman states that the absolute deviation of the American rifle at 550 yards is 11 inches; his work was published in 1848 since which time very considerable improvement has been effected by our best makers. Knowing this to be the case, I instituted some experiments at 500 yards with rifle No. 1: after it had been sighted for that distance ten shots were fired by a friend, who was but a moderate marksman; a reduced cut of the target is given. The distance of each shot from the centre was measured, the sum of the distances was 67·1 inches: the absolute deviation therefore in this experiment was $6\frac{71}{100}$ inches or $2\frac{1}{2}$ inches less than that of the Whitworth rifle.

10.



One-tenth of full size.

This may serve to show how greatly the American rifle, as made by our best gunsmiths, must, and in fact does surpass in accuracy of fire the various rifles used at the present day in Europe, which are confessedly in this respect inferior to the Whitworth. By reference to the tables below it will be seen that the performance of Rifle No. 1 in the above target made at 500 yards, was superior to that of the Swiss rifle at 200 yards.

* "The Rifle," by Hans Busk, M. A., page 94.

No less demonstrable is it, that the American rifle-*pistol*, with a barrel only 12 inches in length, but constructed on the same principle as the larger arm, surpasses in the accuracy of its fire, up to a range of 500 yards, most of the rifles now used in Europe.* This may seem incredible; it is highly *significant* as pointing out, that the principles on which rifled-guns should be constructed, have been better apprehended by our countrymen, than thus far at least, by the ordnance boards of European governments.

Absolute Deviation.

	200 yd.	500 yd.
Enfield,	13·3	28·0 in.
Rifle à tige,	11·0	25·2 "
Swiss,	6·8	
American Government,	6·58	17·3 "

Absolute Deviation of 12 *in. Pistol according to Chapman. Weight of Ball* 100 *to* 115 *grains.*

110 yd.	220 yd.	330 yd.	440 yd.	550 yd.
in.	in.	in.	in.	in.
1·5 to 2	2 to 4	6 to 8	10 to 12	14 to 16

Diameters of Circles containing the best half of the shots.†

	164 yd	328 yd.	492 yd.
Swiss Federal,	5·4 in.	11·40 in.	23
Swiss Chasseur,	8·4 "	22·40 "	40
French Rifle à tige,	12·9 "	26·4 "	40
Austrian Rifle,	26·0 "	40 "	90?
Sardinian Rifle,	22·8 "	38 "	83

Diameters of Circles containing a certain proportion of the shots from the American 12-*inch Pistol.*

220 yd.	275 yd.	440 yd.	500 yd.
5·04 in.	9 inches.	30 in.	18 in.
8 out of 10 shots.‡	8 out of 10.§	All the shots.§	4 out of 10.‖

* Although Gen. Jacob has printed no particulars relative to his rifle practice at distances under 1000 yards, still that it cannot have been remarkably accurate, is patent from the table of the time of the flight of his projectiles given on page 35 of his "Rifle Practice," which is copied without comment by Lieut. Busk, page 165 of his work, and again by Lieut. Wilcox, page 210. By reference to either of these books it will be seen that the time of flight for 100 yards is 325 sec., for 200 yards exactly twice as much, for 300 yards just three times as much, and so on up to 600 yards; which is equivalent to saying that the velocity remained undiminished during a flight of 500 yards! A manifest absurdity, which can only be explained by the surmise that the path of the balls was irregular, and therefore allowed no determination of the time of flight to be made with accuracy. I was first unable to comprehend the meaning of this table, until in experimenting with a ball whose flight was known to be irregular, similar results were obtained. Gen. Jacob himself remarks that the table is a little curious.

† According to Lieut. Wilcox.

‡ Chapman, plate VI.

§ Trials by Mr. Lewis.

‖ An experiment of my own.

Strange indeed is it, that Lieut. Wilcox in his recent treatise on "Rifles and Rifle Practice," while describing each minute variation in the faulty construction of the European arm, should wholly ignore the existence of this most remarkable product of the experimental skill and mechanical ingenuity of his countrymen.

Inasmuch therefore as our own rifle for years has stood without a rival, how happens it that for the use of our army we have been induced to import an inferior arm from France? Were it not better policy to furnish our soldier with the weapon which has became so famous in the hands of our hunters? If greater accuracy or power be required, might it not be well to institute a minute investigation into the causes to which our home-product owes its success, rather than to spend time and incur expense in the study of the inferior rifles of Europe, which although owing their existence to the labors of boards of ordnance, composed of highly educated men, still have never approached in perfection the weapon devised by the *experimental skill* of our American backwoodsmen.

Below are a few of the initial velocities obtained in these experiments. Instead of the average merely, I have given each determination by itself. The powder employed was of very moderate strength, such indeed as is for the most part used in these rifles by our best marksmen:

Rifle No. 2.

Weight of ball 105·01 grains.

Charge.	Initial Vel.
50 grains about 2·15 inches of the bore,	1785 1799 1739
	1774
62 grains, about 2·66 in. the bore.	1890 1925 1925 1928
	1917
75 grains, about 3·22 in. to the bore,	2082 2086
	2084

Rifle No. 3.

Weight of ball 163·04 grains.

Charge.	Initial Vel.
69·4 grs. about 2·3 in. to the bore,	1759 1782 1816
	1785

Rifle No. 4.

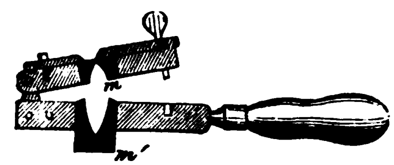
Weight of ball 113·85.

31·5 grs. about 135 in. to the bore,	1141 1136 1177 1057 1136
	1129

Fig. 11 is a section of an universal bullet mould which is per-

haps new. These experiments were greatly facilitated by its use. The portions *m* and *m'* are removable at pleasure. To construct a mould for a new ball, it is merely necessary to make the parts *m* and *m'*, all other labor being spared: an important consideration, where new forms of balls must be made by the dozen.

11.



Troy, February, 1860.

ART. IV.—*On the Conservation of Force;* by Prof. JOSEPH HENRY, Smithsonian Institution.*

[THE following remarks upon the conservation of force, particularly in relation to organic matter, by Professor Henry, Secretary of the Smithsonian Institution, will be interesting to those who have given attention to articles on the same subject, which have already appeared in this Journal.

They are extracted from the Agricultural Report of the Patent Office for 1857.]

Organic Molecules.—"The groups of atoms which we have thus far been considering, are principally those which have been formed under the influence of what is called the chemical force, and result from the ordinary attraction of the atoms. These are comparatively simple groups; but there is another class of groups of atoms of a much more complex character, and which are formed of new combinations of the ordinary atoms under the influence or, we may say, direction of that mysterious principle called the *vital force.* We are able to construct a crystal of alum from its elements by combining sulphur, oxygen, hydrogen, potassium, and aluminum; but the chemist has not yet been able to make an atom of sugar from the elements of which it is composed. He can readily decompose it into its constituents, but it is impossible so to arrange the atoms artificially, as in the ordinary cases of chemical manipulation, to produce a substance in any respect similar to sugar. When the attempt is made, the atoms arrange themselves spontaneously into a greater number of simpler and smaller groups or molecules than is found in sugar, which is composed of molecules of high order, each containing no less than 34 atoms of carbon, oxygen, and hydrogen.

The organic molecules, or atoms, as they are called, are built up under the influence of the vital principle of inferior groups of simple elements. These organic molecules are first produced in the leaves of the plant under the influence of light, and subsequently go through various changes in connection with the vital process. After they are once formed in this way, they may be

* Communicated by Prof. Henry.

combined and recombined by different processes in the laboratory, and a great variety of new compounds artificially produced from them.

But what is this vital principle, which thus transcends the sagacity of the chemist and produces groups of atoms of a complexity far exceeding his present skill? It is generally known under the name of the vital *force;* but since the compounds which are produced under its influence are subject to the same laws, though differing in complexity, as those produced by the ordinary chemical forces; and since in passing from an unstable to a more stable condition in the form of smaller groups, they exhibit, as will be rendered highly probable hereafter, an energy just equivalent to the power exerted by the sunbeam, under whose influence they are produced, it is more rational to suppose that they are the result of the ordinary chemical forces acting under the *direction* of what we prefer to call the vital *principle.* This is certainly not a *force*, in the ordinary acceptation of the term, or in that in which we confine this expression to the attractions and repulsions with which material atoms appear to be primarily endowed. It does not act in accordance with the restricted and uniform laws which govern the forces of inert matter, but with forethought, making provision far in advance of the present condition for the future development of organs of sight, of hearing, of reproduction, and of all the varied parts which constitute the ingenious machinery of a living being. Matter without the vital influence may be compared in its condition to steam which, undirected, is suffered to expend its power in producing mechanical effects on the air and other adjacent bodies, marked with no special indications of design; while matter under its influence may be likened to steam under the directing superintendence of an engineer, which is made to construct complex machinery and to perform other work indicative of a directing intelligence. *Vitality*, thus viewed, gives startling evidence of the immediate presence of a direct, divine and spiritual essence, operating with the ordinary forces of Nature, but being in itself entirely distinct from them.

This view of the subject is absolutely necessary in carrying out the mechanical theory of the equivalency of heat and the correlation of the ordinary physical forces. Among the latter, vitality has no place, and knows no subjection to the laws by which they are governed.

All the constituents of organic bodies are formed of organic molecules, and, as we have said, these are of great complexity, and are readily disturbed and resolved into a greater number of lesser groups. Thus, the constitution of cane sugar is represented by $C_{12}H_{11}O_{11}$, making in all 34 atoms. Organic bodies are, therefore, in what may be called a state of power, or of tottering

equilibrium, like a stone poised on a pillar, which the slightest jar will overturn; they are ready to rush into closer union with the least disturbing force. In this simple fact is the explanation of the whole phenomena of fermentation, and of the effect produced by yeast and other bodies, which being themselves in a state of change, overturn the unstable equilibrium of the organic molecules, and resolve them into other and more stable compounds. Fermentation, then, consists simply generally in the running down from one stage to another of organic molecules, changing their constitution, and at last arriving at a neutral state. There is, however, one fact in connection with the running down of the organic molecules which deserves particular attention, namely, that it must always be accompanied with the exhibition of power or energy, with a disturbance of the ethereal equilibrium in the form of heat, sometimes even of light, or perhaps of the chemical force, or of that of the nervous energy, in whatever form of motion the lattter may consist. It is a general truth of the highest importance in the study of the phenomena of Nature, that whenever two atoms enter into more intimate union, heat, or some form of motive power, is always generated. It may, however, be again immediately expended in effecting a change in the surrounding matter, or it may be exhibited in the form of one of the radiant emenations.

Balance of Nature.—The term balance of organic nature was first applied, we think, by Dumas, to express the relations between matter forming animals and vegetables, and the same matter in an inert condition. We shall apply the term "balance of nature," in a more extended sense, and include within it the balance of power, as well as the transformations of matter. The amount of matter in the visible universe is supposed to remain the same, though it is subject to various transformations, and appears under various forms—now built up into organic molecules, and now again resolved into the simple inorganic compounds. The carbon and other materials absorbed from the air by the plant is given back to the atmosphere by the decaying organisms, and thus what may be called a constant balance is preserved. But this balance, if we may so call it, does not alone pertain to the matter, but also to the energy which is employed in producing these changes. It may disappear for a while, or may be locked up in the plant or the animal, but is again destined to appear in another form, and to exert its effects, perhaps in distant parts of celestial space.

To give precision to our thoughts on this subject, let us suppose that all the vegetable and animal matter which now forms a thin pellicle at the surface of the earth were removed—that nothing remained but the germs of future organisms buried in the soil and ready to be developed when the proper influences

were brought to bear upon them. Let us further suppose the sun to cease giving emanations of any kind into space. The radiation from the earth, uncompensated by impulses from the sun, would soon reduce the temperature of every part of the surface to at least 60° below zero; all the matter and liquid substances capable of being frozen would be reduced to a solid state; the air would cease to move, and universal stillness and silence would prevail.

Let us now suppose that the sun were to give forth rays of heat alone; these would radiate in every direction from the celestial orb, and an exceedingly small portion of them, in comparison with the whole, would impinge against the surface of our distant planet, would melt the ice first on the equator, then on the more northern and southern parts of the globe, and, finally, their genial influence would be felt at the poles. The air would be unequally rarefied in the different zones, the winds would again be called forth, vapor would rise from the ocean, clouds would be formed, rain would descend, and storms and tempests would resume their sway.

If the sun should again intermit its radiation, all these motions would gradually diminish, and after a time entirely cease; the heat given to the earth would, in part, be retained for awhile, but in time would be expended; the water would slowly give out its latent caloric and be again converted into ice. Something of this kind takes place in the northern and southern parts of the earth during the different periods of summer and winter. Since the mean temperature of the earth does not vary from year to year, it follows that all the excess of heat of summer received from the sun is given off in winter and hence the impulses from this luminary which constitute all the energy, producing the changes on the surface of the earth, merely lingering for awhile, are again sent forth into celestial space, changed, it may be, in form, but not in the amount of their power. The solar vibrations have lost none of their energy, for the water has returned to the state of ice, and the surface of the earth is again in the same condition in which it was before it received the solar impulse. The energy of the solar vibrations communicated to the ice overcomes its cohesion, converting it into the liquid state, and the ice again becoming solid gives out the same amount of heat in a less energetic form. Even the motive power of the wind is expended by the friction of its particles in producing an amount of heat equivalent to that which gave rise to its motion, and this also is radiated into celestial space.

But the most interesting part of our inquiry relates to the effects which the radiation alone of heat from the sun would have on the vegetable germs buried in the soil. If these germs were enclosed in sacs filled with starch and other organic ingre-

dients, stored away for the future use of the young plant, as in the case of the tuber of the potato, or the fleshy part of the bean, as soon as the sun penetrated beneath the surface in sufficient degree to give mobility to the complex organic molecules of which these materials consist, the proper degree of moisture also supposed to be present, germination would commence. The young plant would begin to be developed, would strike a rootlet downward into the earth, and elevate a stem towards the surface furnished with incipient leaves. The growth would continue until all the organic matter in the tuber or sac was exhausted; the further development of the plant would then cease, and in a short time decay would commence.

But let us dwell a few minutes longer on the condition of the plant and the tuber before the downward action becomes the subject of consideration. If we examine the condition of the potato which was buried in the earth, we shall find remaining of it nothing but the skin, which will probably contain a portion of water. What has become of the starch and other matter which originally filled this large sac? If we examine the soil which surrounded the potato, we do not find that the starch has been absorbed by it; and the answer which will, therefore, naturally be suggested is, that it has been transformed into the material of the new plant, and it was for this purpose originally stored away. But this, though in part correct, is not the whole truth; for if we weigh a potato prior to germination, and weigh the young plant afterwards, we shall find that the amount of organic matter contained in the latter is but a fraction of that which was originally contained in the former. We can account in this way for the disappearance of a *part* of the contents of the sac, which has evidently formed the pabulum of the young plant. But here we may stop to ask another question: By what power was the young plant built up of the molecules of starch? The answer would probably be, by the exertion of the vital force; but we have endeavered to show that vitality is a *directing principle*, and not a mechanical power, the expenditure of which does work. The conclusion to which we would arrive will probably now be anticipated. The portion of the organic molecules of the starch, &c., of the tuber, as yet unaccounted for, has run down into inorganic matter, or has entered again into combination with the oxygen of the air, and in this running down, and union with the oxygen, has evolved the power necessary to the organization of the new plant.

The oxygen of the atmosphere penetrates into the interior of the potatoe, to enter into combination with the gluten and starch;—or, in other words, to burn it by a slow combustion; and the carbonic acid and water produced find their way, in turn, back to the atmosphere. We see from this view that the

starch and nitrogenous materials, in which the germs of plants are imbedded, have two functions to fulfill—the one to supply the pabulum of the new plant, and the other to furnish the power by which the transformation is effected, the latter being as essential as the former. In the erection of a house, the application of mechanical power is required as much as a supply of ponderable materials.

To return to our first supposition. We have said (and the assertion is in accordance with accurate observation) that the plant would cease to increase in weight under the mere influence of heat, however long continued, after the tuber was exhausted. Some slight changes might, indeed, take place; a small portion of pabulum might be absorbed from the earth; or one part of the plant might commence to decay, and thus furnish nourishment to the remaining parts; but changes of this kind would be minute, and the plant, under the influence of heat alone, would, in a short time, cease to exist.

Let us next suppose the sun to commence emitting rays of *light*, in addition to those of heat. These, impinging against the earth, would probably produce some effects of a physical character; but what these effects would be we are unable, at the present time, fully to say. We infer, however, that the light, not immediately reflected into space, would be annihilated; but this could not take place without communicating motion to other matter. It would probably be transformed into waves of heat of feeble intensity.

Let us now suppose, in addition to heat and light, the chemical rays to be sent forth from the sun. These would also produce various physical changes, the most remarkable of which would be in regard to the plant.

The carbonic acid of the atmosphere, in contact with the expanding surface of the young leaves, would be absorbed by the water in their pores, and in this condition would be decomposed by the vibrating impulses which constitute the chemical emanation. The atoms of carbon and oxygen, of which the carbonic acid is composed, would be forcibly separated; the atoms of oxygen would be liberated in the form of gas, and the carbon be absorbed to build up, under the directing influence of vitality, the woody structure of the plant. In this condition the pabulum of the plant is principally furnished by the carbonic acid of the air, while the impulses of the chemical ray furnish the primary power by which the decomposition and the other changes are effected. This is the general form of the process, leaving out of view minute changes, actions and reactions, which must take place in the course of organization.

In the decomposition of the carbonic acid by the chemical ray, a definite amount of power is expended, and this remains, as it

were, locked up in the plant so long as it continues to grow; but when it has reached its term of months or years, and some condition has been introduced which interferes with the balance of forces, then a reverse process commences, the plant begins to decay, the complex organic molecules begin to run down into simpler groups, and then again into carbonic acid and water. The materials of the plant fall back into the same combinations from which they were originally drawn, and the solid carbon is returned in the form of a gas to the atmosphere, whence it was taken. Now, the power which is given out in the whole descent, is, according to the dynamic theory, just equivalent to the power expended by the impulse from the sun in elevating the atoms to the unstable condition of the organic molecules. If this power is given out in the form of vibrations of the ethereal medium constituting heat, it will not be appreciable in the ordinary decay, say of a tree, extending, as it may, through several years; but if the process be rapid, as in the case of combustion of wood, then the same amount of power will be given out in the energetic form of heat of high intensity. This heat will again radiate from the earth; and in this case, as in that we have previously considered, the impulse from the sun merely lingers for a while upon the earth, and is then given back to celestial space, changed in form, but undiminished in quantity. It may continue its radiating course through stellar space, until it meets planets of other systems; but to attempt to trace it further would be to transcend the limits of inductive reason, and to enter those of unbridled fancy.

In the process we have described, the carbon, hydrogen, and other substances which are absorbed from the atmosphere, are returned to this great reservoir to be used again, and, it may be, to undergo the same changes many times in succession. The earthy materials are again returned to the earth, and all the conditions, as far as the individual plant which we are considering is concerned, are the same as they were at the beginning. The absorption of power in the decomposition of the carbonic acid gas, and its evolution again when the recomposition is produced of the same atoms, is precisely analogous to that which takes place in forcibly separating the poles of two magnets, retaining them apart for a certain time, and suffering them to return by their attractive force to their former union. The energy developed in the approach of the magnets towards each other is just equal to the force expended in their separation.

By extending this reasoning to the vast beds of coal which are stored away in the earth, we are brought irresistibly to the conclusion that the power which is evolved in the combustion of this material, now so valuable an agent in the processes of manufacture and locomotion, is merely the equivalent of the force which was expended in decomposing the carbonic acid which

furnished the carbon of the primeval forests of the globe; and that the power thus stored away millions of years before the existence of man, like other preordinations of Divine Intelligence, is now employed in adding to the comforts and advancing the physical and intellectual well-being of our race.

In the germination of the plant a part of the organized molecules runs down into carbonic acid to furnish power for the new arrangement of the other portion. In this process no extraneous force is required; the seed contains within itself the power and the material for the growth of the new plant up to a certain stage of its development. Germination can, therefore, be carried on in the dark, and, indeed, the chemical ray which accompanies light retards rather than accelerates the process. Its office is to separate the atoms of carbon from those of oxygen in the decomposition of the carbonic acid, while that of the power within the plant results from the combination of these same elements. The forces are therefore antagonistic, and hence germination is more rapid when light is excluded; an inference borne out by actual experiment.

Animal Organism.—Besides plants, there is another great class of organized beings, viz: animals; and as we commenced with the consideration of the seed in the first case, let us begin in this with the egg. This, as is well known, consists of a sack or shell containing a mass of organized molecules formed of the same elements of which the plant is composed, viz: carbon, hydrogen, oxygen, and nitrogen, with a minute portion of sulphur and other substances. Without attempting to describe the various transformations which take place among these organized molecules, a task which far transcends our knowledge or even that of the science of the day, we shall merely consider the general changes which occur of a physical character.

As in the case of the seed of the plant, we presume that the germ of the future animal pre-exists in the egg, and that by subjecting the mass to a degree of temperature sufficient perhaps to give greater mobility to the molecules, a process similar in its general effect to that of the germination of the seed commences. Oxygen is absorbed through some of the minute holes in the shell, and carbonic acid constantly exhaled from others. A portion then of the organic molecules begins to run down, and is converted into carbonic acid, and, possibly, water. During this process power is evolved within the shell—we cannot say, in the present state of science, under what particular form; but we are irresistibly constrained to believe that it is expended under the direction, again, of the vital principle, in rearranging the organic molecules, in building up the complex machinery of the future animal, or developing a still higher organization, connected with which are the mysterious manifestations of thought and volition.

In this case, as in that of the potato, the young animal, as it escapes from the shell, weighs less than the material of the egg previous to the process of iucubation. The lost material in this case, as in the other, has run down into an inorganic condition by combining with oxygen, and in its descent has developed the power to effect the transformation we have just described.

We have seen, in the case of the young plant, that after it escapes from the seed, and expands its leaves to the air, it receives the means of its future growth principally from the carbon derived from the decomposition of the carbonic acid of the atmosphere, and its power to effect all its changes from the direct vibratory impulses of the sun. The young animal, however, is in an entirely different condition; exposure to the light of the sun is not necessary to its growth or existence; the chemical ray, by impinging on the surface of its body, does not decompose the carbonic acid which may surround it, the conditions necessaary for this decomposition not being present. It has no means by itself to elaborate organic molecules, and is indebted for these entirely to its food. It is necessary, therefore, that it should be supplied with food consisting of organized materials, that is, of complex molecules in a state of unstable equilibrium, or of power. These molecules have two offices to perform: one portion of them, by their transformations, is expended in building up the body of the animal, and the other in furnishing the power required to produce these transformations, and, also, in furnishing the energy constantly expended in the breathing, the pulsations, and the various other mechanical motions of the living animal. We may infer from this that the animal, in proportion to its weight before it has acquired its growth, will require more food than the adult, unless all its voluntary motions be prevented; and secondly, that more food will be required for sustaining and renewing the body when the animal is suffered to expend its muscular energy in labor or other active exercise.

The power of the living animal is immediately derived from the running down of the complex organized molecules, of which the body is formed, into their ultimate combination with oxygen, in the form of carbon, water and ammonia. Hence, oxygen is constantly drawn into the lungs, and carbon is constantly evolved. In the adult animal, when a dynamic equilibrium has been attained, the nourishment which is absorbed into the system is entirely expended in producing the power to carry on the various functions of life, and to supply the energy necessary to perform all the acts pertaining to a living, sentient, and, it may be, thinking being. In this case, as in that of the plant, the power may be traced back to the original impulse from the sun, which is retained through a second stage, and finally given back again to celestial space, whence it emanated. All animals are constantly radiating heat, though in different degrees, the amount

in all cases being in proportion to the oxygen inhaled and the carbon exhaled. The animal is a curiously contrived arrangement for burning carbon and hydrogen, and the evolution and application of power. In this respect it is precisely analogous to the locomotive, the carbon burnt in the food and in the wood performing the same office in each. The fact has long been established, that power cannot be generated by any combination of machinery. A machine is an instrument for the application of power, and not for its creation. The animal body is a structure of this character. It is admirably contrived, when we consider all the offices it has to perform, for the purpose to which it is applied, but it can do nothing without power, and that, as in the case of the locomotive, must be supplied from without. Nay, more, a comparison has been made between the work which can be done by burning a given amount of carbon in the machine, man, and an equal amount in the machine, locomotive. The result derived from an analysis of the food in one case, and the weight of the fuel in the other, and these compared with the quantity of water raised by each to a known elevation, gives the relative working value of the two machines. From this comparison, made from experiments on soldiers in Germany and France, it is found that the human machine, in consuming the same amount of carbon, does four and a half times the amount of work of the best Cornish engine. The body has been called "the house we live in," but it may be more truly denominated the machine we employ, which, furnished with power, and all the appliances for its use, enables us to execute the intentions of our intelligence, to gratify our moral natures, and to commune with our fellow beings.

This view of the nature of the body is the furthest removed possible from materialism; it requires a separate thinking principle. To illustrate this, let us suppose a locomotive engine, equipped with steam, water, fuel—in short, with the potential energy necessary to the exhibition of immense mechanical power; the whole remains in a state of dynamic equilibrium, without motion or signs of life, or intelligence. Let the engineer now open a valve which is so poised as to move with the slightest touch, and almost with volition, to let on the power to the piston; the machine now awakes, as it were, into life. It rushes forward with tremendous power, it stops instantly, it returns again, it may be, at the command of the master of the train; in short, it exhibits signs of life and intelligence. Its power is now controlled by mind—it has, as it were, a soul within it. The engine may be considered as an appendage or a further development of the body of the engineer, in which the boiler and the furnace are an additional capacious stomach for the evolution of power; and the wheels, the cranks and levers, the bones, the sinews, and the muscles, by which this power is applied.

Washingtou, D. C., 1860.

ART. V.—*On a mode of employing Instantaneous Photography as a means for the Accurate Determination of the Path and Velocity of a Shooting Star, with a view to the Determination of its Orbit;* by JONATHAN H. LANE.

A tolerably accurate knowledge of the orbits of those meteors, or shooting stars, which may enter our atmosphere, would be of very high value in the settlement of certain questions as to their origin. Hitherto this knowledge has appeared unattainable by reason of the difficulty of effecting sufficiently precise observations of the meteor in the transient period of its visible flight, especially considering how this difficulty is aggravated on account of retardation of its motion by the resistance of the atmosphere. Recently, however, a method has occurred to me of applying instantaneous photography, so as to show accurately, not only the track of the meteor, but the division of its track into many equal and known fractions of time. If this can be successfully accomplished, we should have the data for ascertaining the velocity of the meteor at each point of the recorded part of its track, and the rate or law of variation of the velocity, and thence, with probably a good degree of accuracy, the velocity it had beyond the limits of the atmosphere, and the like remark may be made concerning the direction of the motion, should that be found subject to change.

The basis of the proposed process, as already intimated, is the extraordinary advances that have been made within a few years in the preparation of the sensitized surfaces of photographic plates, whereby artists are enabled to produce good pictures by an exposure of a very small fraction of a second—so small as to afford a tolerable definition of objects in motion, such as sailing vessels. This holds out encouragement for a hope, at least, that a passing meteor would leave a visible trace on a plate so prepared, or, even if that degree of sensitiveness has not yet been reached, that it will be hereafter. I need therefore make no apology for placing the suggestion on record previous to direct experiment on this point.

In the first place, simple exposure in a camera, at a given station, would give the apparent track of a meteor as seen by the observer at that station, and a pair of such records made in two cameras at two stations, would give the track in absolute space. In the second place, if one of the two cameras were furnished with a mechanism by which equidistant points of time should be marked upon the trace made in that camera, these points could be referred to the real path in space, and if both cameras were in like manner furnished, the two records would, to that extent be a check upon each other, and serve to reduce the limits of probable error. The

device for marking time is an application of the revolving glass prism, very similar to that described in my paper on a visual method of comparing time between distant stations, published in the January number of this Journal.* Immediately in front of the object glass of the camera, a glass prism of small angle and sufficient area to cover the entire aperture, is made to rotate at an accurately measured rate of say twenty-five revolutions per second. The prism may be replaced by an excentric lens, or the object glass itself may revolve on a slightly excentric axis. The consequence will be that the image of a fixed star in any part of the field of view will traverse the circumference of a circle every twenty-fifth of a second, and the image of a shooting star will combine this motion with its motion of translation. If the photographic surface retain a visible impression of the looped curve or the waved curve which will thus be produced, then, neglecting for the present the small effects of optical distortion, the line drawn midway between the two straight or regularly curved lines between which the looped or waved curve oscillates, will represent the apparent track of the meteor, and the points where it intersects the looped or waved curve, if they be translated along this middle line through a space equal to the optical displacement of the meteoric image, will show the apparent place occupied by the meteor at points of time separated by the equal intervals of one fiftieth of a second. If the period be made too brief, the impression left by the head of the meteor in one sweep of the looped or waved curve, might possibly be obliterated by the impression of the closely following parts of its train, while the head is traversing the subsequent sweeps of the curve. But there is no reason to anticipate from this cause any difficulty in obtaining a sufficiently short period to determine the law of variation of the velocity or direction.

In the above statement I have supposed only a single camera, but it will probably be impossible in this way to command a sufficient extent of the heavens. A system of many cameras may, however, be formed, so arranged that their several optic axes shall cross in a common point *in front* of the object glasses. The object glasses may thus be approximated as closely as we can desire, and the several revolving prisms, or excentric lenses, may have a common geared connection, and the backs of the cameras will be readily accessible for the renewal of plates. When the track of a meteor, by reason of its extent or situation, is obtained in parts from different cameras of such a system, it is geometrically impossible, on account of the spherical excess, that the exact interval of one fiftieth of a second between the times

* Vol. xxix, 43.

marked upon the meteor's track, should, in general, be preserved in the transition from one plate to another in all situations of the track, or in other words, that every two adjacent cameras in the system shall be capable of marking, in the manner described, the same common point of time upon the track of a meteor, but the exact difference in time can always be known.

In the execution of such a plan as this, two stations are to be selected at a suitable distance, and a system of cameras established at each, of such range that the two may cover in common a sufficient extent of the upper regions of the atmosphere to afford a fair chance for the occurrence of meteors. Each station will require at least an observer and a photographer. The photographer will renew the plates as often as their surfaces, either from time or exposure, become impaired, and will perform the manipulations required in fixing the impression when taken. The observer, after having made the necessary instrumental adjustments and determinations will be charged with the sole duty of watching for meteors in the region covered by his system of cameras, and at the appearance of a meteor will touch a spring so contrived as to cause the instant unveiling of all the cameras of the system, and on the extinction of the meteor will promptly replace the screen.

The expense and trouble of this process will certainly be great, but will not be disproportioned to the importance of the object in view. Only let us have a photographic surface that will give a visible trace of the meteor's path, in the face of exposure to the light of the sky during the time of the meteor's visible flight, and then success, as regards the attainments of an accurate record, will be nearly certain, and we should not hesitate at the expense and trouble.

Nor does it seem to me that our success would be much less certain in respect to the reliable determination of the direction and velocity which the meteor had before entering the atmosphere, and consequently of the orbit in which it had moved. A very simple calculation based upon the mechanical theory of heat, leads us to the conclusion that any body of a nature to become readily incandescent by heat, of such a thickness as half an inch, and not possessing a greater power of conducting heat through its mass than any we are acquainted with, must, on entering our atmosphere with planetary velocity, become self luminous by the time that velocity has been reduced by some such fractional part as one thousandth. The *vis viva* of a body moving with a velocity of twenty miles a second is equivalent to the heat that would raise the temperature of an equal weight of water about 224,000° Fah. With such a velocity, so many times exceeding that of sound, the masses of air lying in the

path of the body must be driven before it, and receive a velocity equal to that of the body, or at least to a large fractional part of it. The mass of air which the body must have encountered in losing the thousandth part of its velocity, will, therefore, be of the order of a thousandth part of that of the body. With the loss of a thousandth part of the velocity, the loss of the body's own *vis viva* will correspond to the quantity of heat that would raise the temperature of its weight of water 448° Fah. If after making allowance for the motion communicated to the displaced air—approximately one half—and for the quantity of generated heat which this air retains and carries off, we assume that a twentieth part of the above 448° enters into the body itself, and by reason of the rapidity of its production is collected in a superficial coating of a hundreth part of its mass, and give this a specific heat within that of water, we should find an elevation of temperature of 2240° or upwards. The inference we would draw from these considerations seems confirmed by what we know of the great length of the visible flight of meteors, and of the great elevation of the region of atmosphere in which it occurs.

If, therefore, upon suitable trials made upon the fixed stars, and upon shooting stars themselves, we shall find ourselves in possession of sufficient photographic power, there is no reason why an organized system of observations should not be instituted. If the fact in regard to the retardation of a meteor's motion be as the foregoing considerations lead us to anticipate, the discussion of a collection of such records as we should obtain, of a large number of meteors, will be likely to afford us complete assurance on the subject, by pointing out certain laws of the resistances at different altitudes. A moderate degree of accuracy in the absolute determination of the orbits, except, when they make a near approach to the parabola, will be sufficient to answer all the questions of interest that will be likely to arise upon which a knowledge of the orbits would have any bearing. Whether the November meteors, for instance, move through regions that would identify them with the Zodiacal light, according to the theory of the late Prof. Olmsted, is a question that would receive an absolute determination.

ART. VI.—*The True Figure of the Earth.*—Notice by Mädler in Prof. Heis' Wochenschrift für Astronomie, Meteorologie und Geographie, No. 51 and 52. Dec. 21 and 28, 1859.*

Essai d'une détermination de la véritable figure de la terro. Par T. F. DE SCHUBERT. St. Petersbourg, 1859.

THIS brief but very important paper treats of a question which has engaged mankind for ages, and treats it in such a way as to convince us that an essential step forward has been taken towards its final determination.

It is not intended to recapitulate the history of opinions and notions which antiquity, as well as the middle ages, had formed on this topic so generally interesting, as it does not belong to the plan of the work to be discussed.

Since the establishment of Newton's theory, that the earth is flattened at its poles, and the confirmation of this theoretical result by the first measurements of arcs in the eighteenth century, the chief inquiry has been directed to the magnitude of this compression, or, in other words, the difference between the semi-major and the semi-minor axes of the earth. The attempt was made to determine it by comparing arcs measured in different latitudes, the latitudes of their extremities being astronomically and their extent on the earth geodetically determined. In this manner was obtained, between 1735 and 1746, two arcs, the one in Peru, and the other in Lapland, which could be compared with each other, as well as that previously measured by Cassini in France. The result of the comparison was not satisfactory. Although they proved a comparison, the measures did not agree, and the source of this difference was too obscure to favor the supposition that any nearer approach had been made to the object of research.

In the course of the eighteenth century measures of arcs of the meridian were executed or attempted in France, Austria, Italy and Pennsylvania, at the Cape of Good Hope and other places. Their comparison made it apparent that the compression ($\frac{1}{174}$), as at first concluded, was too great, and that it must be reduced one half or even more. Yet the uncertainty remained very considerable.

The measurement of arcs was continued in the nineteenth century over greater extents in different parts of the earth and with greater care and accuracy, and the close agreement of these arcs, as it appears from the comparison of their parts, left but little, further to be desired. Perceptibly different values for the compression were nevertheless obtained, as when the most proba-

* Translated for the American Journal of Science by CHARLES A. SCHOTT, Assistant, and communicated by Prof. A. D. BACHE, Supt. U. S. Coast Survey.

ble value was determined from them all, there still remained from the separate measures of arcs, deviations which could not be ascribed to errors of observation.

Repeated investigation has shown that the old measurement of a degree by Maupertuis and Outhier was considerably in error. Rosenberg and Swanberg obtained quite different values. In like manner the measures executed by Boscovich, Le Maire, Liesganig, Beccaria and others in the eighteenth century could not bear severe criticism nor stand side by side with the better and approved arcs. A more accurate and reliable coefficient of compression still remains a desideratum.

We insert here for comparison the values of the compression, as they result, for the most part, from the same measures, but according to different methods of reduction, in the first four decades of this century.

	Semi-major axis in toises.	Semi-minor axis in toises.	Compression in toises.	Coefft. of compression.
Walbek, - -	3271742·8	3260989·3	10785·5	$\frac{1}{302\cdot78}$
Schmidt, - -	3271852·3	3260853·7	10998·6	$\frac{1}{297\cdot48}$
Bessel, (I.), -	3271953·9	3261072·9	10881·0	$\frac{1}{300\cdot70}$

As Bessel afterwards discovered an error in Delambre's French triangulation which could not be without influence on the combined result, he repeated his reduction with this correction and found—

Bessel, (II), -	3272077·1	3261139·3	10937·8	$\frac{1}{299\cdot15}$

Still, according to the last reduction, the probable error is three times as great as the difference of Bessel (I) and Bessel (II).

Since in this even the best measures of this sort do not yield sufficiently accordant results, it was attempted to find the compression in two other ways. The observations of the pendulum give, as a consequence of the earth's compression, different lengths of the seconds pendulum in different latitudes, and hence we can deduce, conversely, the compression from these observed lengths or, if we choose, from the number of beats which the same pendulum makes in the course of a day. Observations made with extreme care were used as the foundation of such experiments, yet still the result ($\frac{1}{288\cdot20}$) fluctuated between the limits, $\frac{1}{285}$ and $\frac{1}{291}$.

The moon's path, on which the compression was not without influence, was also resorted to, and gave $\frac{1}{302}$ for the compression, but on account of the smallness of the aggregate effect it is of inferior accuracy.*

We have found it necessary to present this general view in order to define the limit of the results arrived at by preceding

* This result depends, in a measure, on the law of density of the earth in passing from the surface to the centre.

efforts, and in order to place the merit of the writer in its proper light, especially for those not possessing a special knowledge of the subject in question.

The preceding investigations were all based upon the following assumptions:

(1.) The meridians of the earth are ellipses.*

(2.) The minor axis is also the axis of rotation.

(3.) All the meridians of the earth are equal.

The writer remarks that, in a rigorous sense, no one of these assumptions is proved and that we do not possess the means of proving the first two. We may however add, that these two assumptions, if not absolutely, must be very nearly true.†

In regard to the third assumption we are now prepared to submit it to investigation, and the previous failures compel us to question its applicability. Paucker and Borenius have already attempted to prove that it must be false, but neither has arrived at any definite result. The writer uses as a basis the following measurements of arcs:

(1.) The Russian (or more properly Russo-Scandinavian) executed in 1820–51 by Hansteen, Selander, Struve and Tenner from Fuglenäs (in latitude 70° 40′ 11″ ·3 N) to Staro-Nekrasofka (45° 20′ 02″ ·8 N) the longest arc yet measured.

(2.) The Indian arc, 1802–43, by Lambton and Everest, from Kaliana (29° 30′ 48″ ·9 N) to Punnæ (8° 09′ 32″ ·3 N).

(3.) The French arc, 1792–1806, by Méchain, Delambre, Biot and Arago, from Dunkirk (51° 02′ 08″ ·5 N) to Formentera (38° 39′ 56″ ·1 N).

(4.) The measure at the Cape of Good Hope, by Henderson and Maclear, from 34° 21′ 06″ ·3 S to 29° 44′ 17″ ·7 S.

(5.) The Peruvian arc, 1735–46, by Bouguer and la Condamine, from Tarqui (3° 04′ 32″ ·1 S) to Cotchesqui (0° 02′ 31″ ·4 N).

(6.) The Prussian arc, 1831–34, by Bessel and Bæyer, from Memel (55° 43′ 40″ ·4 N) to Trunz (54° 13′ 11″ ·5 N).

(7.) The British arc, by Roy and Mudge, from Clifton (53° 27′ 31″ ·1 N) to Dunnose (50° 37′ 07″ ·6 N).

(8.) The Pennsylvanian arc, 1764, by Mason and Dixon, from 39° 56′ 22″ ·5 N to 38° 27′ 37″ ·5 N.‡

The writer does not explain why he has not taken in other reliable measures, in particular the Hanoverian by Gauss and the Danish arc by Schumacher; but his principal aim being to prove merely the untenability of the assumption that all meridians are equal and similar, and pronouncing his own work as preliminary, no exception can be taken to this course. In a perfectly rigorous investigation by the method of least squares, where each

* In the recent account of the Ordnance Survey of Great Britain the figure of the meridian, in one of the hypotheses, is not restricted to this condition.

† The truth of the second assumption may be granted.

‡ The reason for taking in this arc will be apparent from what follows.

measure with its corresponding weight would enter, such omissions could of course not be permitted.

Reducing and comparing with each other, each of the eight arcs, he obtains twenty eight binary combinations, which he communicates in detail; but this can only be for the purpose of showing the impossibility of an agreement by the assumption hitherto made. For instance, the Russian arc compared with the English gives $\frac{1}{215{\cdot}7}$, the French compared with the English gives $\frac{1}{379{\cdot}4}$, and the deviations from the general mean are still greater in comparing the Prussian and Pennsylvanian arcs with all the others. It is thence inferred that the meridians of the earth are not equal to each other, and thence that the equator and the parallels are not circles, so that it is generally impossible to draw a great circle on the earth's surface anywhere.

Meridians different in form and length really indicate different polar compressions, a non-agreement of the results found, proving nothing against the accuracy of the measures.

Now since all meridians must converge at the poles they must all have in common one and the same diameter, viz., the smallest (the axis of rotation) which can be obtained from each of the large arcs referred to.

The three greatest measured arcs are—

the Russo-Scandinavian............	$= 25° \; 20' \; 08''{\cdot}5$,
" Indian......................	$= 21 \;\; 21 \;\; 16 \;{\cdot}6$,
" French.....................	$= 12 \;\; 22 \;\; 12 \;{\cdot}4$,

whilst all the others, including those not used by the writer, are less than 5° in extent.

By dividing each of these three arcs into two equal parts and comparing one part with the other and also each part with the whole, the writer obtains the following mean values:

	Semi-major axis (toises).	Semi-minor axis (toises).
From the Russian............	3272610·3	3261428·7
" " Indian.............	3272650·9	3261547·4
" " French............	3273448·2	3260364·7

In the first two, the differences are unimportant, but the last shows a greater deviation. Schubert remarks that this latter deviation can be got rid of by changing the latitude of Carcassonne, the selected point of division of the French arc, by 1''·96, which may not exceed the limits of uncertainty. He considers it safer, however, to obtain the semi-minor axis from the first two arcs alone, the greater arc (the Russian) having twice the weight of that of India. In this manner he deduces the semi-minor axis:

3261467·9 toises.

By means of this value the semi-major axis can be found for each measure of an arc. It results as follows:

For the meridian of Kaliana in Long. 95° 20′	3272581·3
" " " Dorpat " " 44° 23′ 10″	3272650·1
" " " Tarqui " " 298° 44′	3272382·8

Three radii and their included angles suffice for the determination of the ellipse. The writer finds for the semi-major axis of the equatorial ellipse 3272671·5 toises
and its direction 58° 44′
for the semi-minor axis of the equatorial ellipse 3272303·2
and its direction 148° 44′

Compression of the equator.............. = $\frac{1}{8886}$;
Polar compression of the greatest meridian = $\frac{1}{302\cdot100}$
" " " smallest " = $\frac{1}{302\cdot004}$,

and the separate arcs calculated with these values have the following deviations:

	in toises.	in arc.
Peru,.....................	+ 1·2	0″·077
Pennsylvania,...............	−105·68	6 ·687
England,..................	+ 11·68	0 ·736
France,...................	− 25·45	1 ·607
Cape of Good Hope,..........	− 6·98	0 ·442
Prussia,....................	+ 23·28	1 ·267
Russia,....................	− 20·47	1 ·289
India,....................	+ 25·54	1 ·619

In reference to the arcs showing greater deviations the writer remarks that the Pennsylvanian arc was measured with imperfect means (by the chain only, and without triangulation) and that he has taken it merely for trial.* The other seven deviations are so small that no one exceeds the ten thousandth part of the measured length, and the great accuracy of the Peruvian measure, which had been already shown in former discussions, appears here in its true excellence. Honor is justly due to the memory of the men who, at so early a period, accomplished such a work under the untold difficulties of ten years' separation from all civilized countries.

It has frequently been shown that the geodetic measures, in particular the longitudes, do not harmonize with astronomical observations, but no one has yet succeeded in discovering the cause of this disagreement. The ellipticity of the earth's equator, as discovered by the writer, will call forth new investigations,

* Dr. Maskelyne in his description of Mason and Dixon's base (Phil. Trans., London, 1768) says they employed *rods of fir* frequently compared with a standard brass measure at a fixed temperature. This line was revised in 1849 and 1850 by Lt. Col. Graham, U. S. Top. Eng. See Message of the Gov. of Md., &c., in relation to the intersection of the boundary lines of the States of Maryland, Pennsylvania and Delaware. Washington, 1850.

since it shows that each geodetic survey, and particularly the longitudinal differences, require correction. The writer shows from two examples how we must proceed and what we may expect.

Between Pulkowa and Dorpat the difference of longitude was found as follows:

Astronomical	3° 36′ 16″·77
Geodetic	3 36 23 ·07′
Difference	+6″·94 = 57·9 toises.

The correction now found to the geodetic is—

−6″·01 = 50·1 toises.

and the remaining error = 0″·93 = 7·8 toises.

Between Pulkowa and Warsaw is found—

Astronomical	9° 17′ 48″·43
Geodetic	9 18 01 ·24
Difference	+12″·81 = 124·7 toises.
Correction to the geodetic	−18 ·85 = 183·4 toises.
remaining error	= − 6 ·04 = 58·7 toises.

The first error is thus completely got rid of and the second is reduced to nearly one half.

The writer concludes with the following remarks:

"This determination of the earth's figure is only an approximation, subject to many improvements, when we have more data, and when we use more rigorous methods of reduction. But it shows that we can obtain an agreement of the measures in which we have not heretofore succeeded. The determination of the general figure of the earth does not exclude local irregularities of its surface.

When this hypothesis of the earth's figure, after a scrutinizing investigation, finds general adoption, all geographical positions determined by geodetic means must have corrections applied, which will refer principally to the differences of longitude."

As much as I may agree with this view and as certainly as the importance of the object requires the application of rigorous methods, it is yet clear that the trouble will be sufficiently rewarded when further reliable data, particularly from the western hemisphere, shall be available to the computer. It may be remarked here that the meridian in the western hemisphere corresponding to the small equatorial axes passes through Newfoundland, and, in the eastern, through the Amoor country and Eastern Siberia. For these countries we have at present not even approximate determinations. The Pennsylvania measure of 1764 is worthless for accurate investigation and the superior Peruvian measure is at too great a distance. Measurements of

arc in the meridian of Irkutsk and extending through Mantchooria southwards, through the Chinese empire, as well as the Atlantic States of the American Union* will give us the smallest meridian of the earth with the same accuracy, corresponding to the determination of the greatest, which passes through Eastern Europe (Kostromo, Stawropol, Erzerûm). Still, much requires to be done. The continuation of the Russian Survey through Turkey, and if possible to Eastern Africa, would be most important; and Struve has already (in 1857) advocated it in the Paris Academy. The opposite part of this greatest meridian passes over the icy deserts of North Western America and the group of islands known as the Marquesas, and hence afford no possibility of its completion on that side.

After this article was printed, the writer received the account of the New British Survey and notice of the correction of a small error of computation. This induced him to reprint some of the sheets on which the numerical results are given as above.

It will hardly be necessary to add anything in praise of the author. To bring up a question of such importance is sufficiently praiseworthy in itself, but the writer has done more; he has opened the way to a final determination and has given it, at least in its general aspect. We shall rejoice if this interesting subject calls other powers into the field to labor further in this direction. Corrections like those which the writer has made for Dorpat and Warsaw will be required for many other places, and each comparison of this kind furnishes its share to the final determination. MÄDLER.

ART. VII.—*On the Transit Instrument as a substitute for the Zenith Telescope in determining Latitude, and on the Latitude of New Haven;* by Prof. C. S. LYMAN.

THE method of determining latitude by measuring micrometrically the difference of meridional zenith distances of stars on opposite sides of the zenith, known commonly as Talcott's method, strongly commends itself to observers, both by its beautiful simplicity, and the very great accuracy of its results. It is the method of late years exclusively used in the operations of the United States Coast Survey, and may properly be regarded as one of the many excellent incidental fruits of that great National work. A special instrument, however, the zenith telescope, has been required for making the necessary observations, and sup-

* It may be stated in connection with this subject that the geodetic surveys of two arcs, one in the New England States, the other on the Chesapeake Bay, are already completed and the astronomical part of the former is nearly so, promising an important result derived from the labors of the U. S. Coast Survey at no distant time.

posed to be indispensable for this purpose. It indeed admirably answers the end for which it was invented, and in respect to simplicity, convenience, and efficiency, leaves almost nothing to be desired. But being expensive, and of limited use, it is likely to be in the hands of but few observers. The suggestion, therefore, of additional instrumental facilities for using this method of latitude, so as to render it more widely available, may not be considered as without value.

At the recent meeting of the American Association, at Springfield, the writer, in a brief communication, pointed out the facility and efficiency with which a transit instrument may be used as a substitute for the zenith telescope, in such observations. The object of the present article is, in part, to exemplify further the same point, and at the same time to place on record the results of a more careful determination, astronomically, of the latitude of New Haven, than, so far as appears, has heretofore been attempted.

The essential parts of the zenith telescope, as an instrument for determining latitude by Talcott's method, are its level and micrometer. And the superiority of the method itself rests mainly on the fact that its results depend on these simple and efficient instrumental means, instead of on graduation. It is obvious, that a transit instrument with a declination micrometer and a suitable level attached to its finding circle, is, at the same time, essentially a zenith telescope, and is capable of performing the whole work of that instrument—as accurately, if of corresponding size and quality, and as conveniently, if furnished with a reversing apparatus. The advantages of such a use of the transit instrument, in a multitude of cases, scarcely need to be pointed out. Where both latitude and longitude are to be determined at the same station, as is usually the case, a single instrument will suffice for both, and thus half the ordinary equipment for the purpose may be dispensed with. The corresponding diminution of expense will be a consideration of weight where pecuniary means are limited, as will also the saving of transportation, in the case of boundary and other surveys in remote and uninhabited districts. The observer who has at command such an instrument, even of very moderate size, will have it in his power to fix his latitude with a degree of precision scarcely surpassed even in first class observatories.

The transit instrument with which the observations were made, the results of which are given in this article, has an object glass (by Fitz) of two and six-tenths inches aperture, and thirty-five and a half inches focal length; a filar-micrometer, so constructed as to be used with equal facility either in the plane of the meridian or perpendicular to it; and a twelve inch circle, (graduated on the new and excellent engine of Messrs. E. & G. W.

Blunt, of New York,) reading by two verniers to 10″. The level attached to the alidade of the circle has a run of one-thirteenth of an inch for 5″; and this being the smallest division of the scale, single seconds and fractions have to be estimated by the eye—a defect which will be further noticed in another connection.

The graduation of the circle is very accurate. The difference between the readings of the opposite verniers seldom amounts to to 15″, and from its law of variation, it is manifestly due, in the main at least, to a very slight excentricity in centering the circle on the graduating engine, not to imperfection in the engine itself.

The micrometer-screw has 75 threads to the inch, or one revolution of the head equal to 78″·74.

The optical performance of the instrument is very satisfactory. Transits of Polaris are readily taken at midday, and in favorable states of the atmosphere, at that hour, even by reflection in mercury. Transits of Aldebaran have been taken when the star was within 6° of the sun's center. The eye-piece commonly used is a diagonal one of four lenses, with prismatic reflector,—the magnifying power about 190.

The method of observing for latitude with the transit instrument is of course essentially the same as with the zenith telescope. A pair of stars having been selected, which culminate within a few minutes of each other, but on opposite sides of the zenith, and having their zenith distances nearly equal, the instrument is set for the star first to culminate, the level clamped, the bubble brought to zero by the slow-motion screw which turns the whole instrument, the star bisected with the micrometer wire, and the readings of the level and micrometer noted. The axis is then reversed, the bubble brought to zero as before, and the other star observed in the same manner.

The pairs for the observations now to be considered, were taken from the Catalogue of the British Association. The stars are to the sixth magnitude, and mostly within 25° of the zenith, the difference of zenith distances of the stars of a pair being usually less than 25′—averaging about 12′.

The individual results of 92 pairs are exhibited in the following table, which includes, with two exceptions, all the observations made, whether in favorable or unfavorable states of the atmosphere. In a few cases where the B. A. C. differs widely from other catalogues, the B. A. C. results are enclosed in brackets [ ] and are omitted in computing the mean. That the fault is in the B. A. C., is obvious from the accordance of the corresponding results from other catalogues with the general average.

The contents of the table are as follows:

Col. 1 and 2. The numbers from the British Association Catalogue designating the stars of a pair.

3. The dates of the observations.

4. The mean results for latitude from each pair, using the positions of the B. A. C.; the seconds of the individual results, where the same pair was observed on different evenings, being placed at the left hand side of the column, and connected with their mean by a brace.

5. The differences, Δ, between these mean results of pairs and the mean of the whole.

6. The results for latitude from the same observations, the position of one or both the stars of the pair being taken from some other catalogue than the B. A. C.; the arrangement as in column 4.

7. The names of these other catalogues; G. standing for Greenwich Twelve Year Catalogue; B. that of the British Association; R. the catalogue in Radcliffe Observations for 1856; and A. the English Nautical Almanac. A. G., for example, signifies that the position of the first star of the pair is from the Nautical Almanac, and that of the other from the Twelve Year Catalogue; etc.

8. The differences between the results in column 6 and the mean of the whole.

Number of each Star in B. A. C.		Date of observation.	Lat. by B. A. C.		Δ	Lat. by other catalogues.		Name of catalogue.	Δ
				41° 18′			41° 18′		
121	164	1858, Nov. 8	43″·4	43″·7	+1·3	42″·3	42″·6	B. G.	+0·2
"	"	" " 11	44 ·1			43 ·0			
155	219	" " 10		40·0	−2·4		39·8	G. B.	−2·6
245	334	" " 11		42·1	−0·3		42·0	B. G.	−0·4
267	314	" Dec. 16		45·3	+2·9		45·3	B. G.	+2·9
334	487	" Nov. 10	42·1	42·5	+0·1	41·7	42·2	G. G.	−0·2
"	"	" " 11	43·0			42·7			
1083	1123	1859, Jan. 20		45·0	+2·6		43·0	R. G.	+0·5
1175	1214	" " "		40·7	−1·7		42·1	B. R.	−0·3
1313	1367	" " "		43·5	+1·1		42.0	R. R.	−0·4
3523	3777	" Oct. 21		41.0	−1·4		40·2	A. A.	−2·2
3742	3767	" May 6		42·6	+0·2		42·4	B. G.	0·0
3767	3809	" " "		42·6	+0·2		42·4	G. B.	0·0
3856	3913	" " "		41·8	−0·6		42·2	G. R.	−0·2
3968	3995	" " "		40.8	−1·6		40·5	B. G.	−1·9
4112	4151	" " "		43·9	+1·5		43·0	G. G.	+0·5
4216	4240	" " "		42·7	+0·3				
4351	4371	" " "		42·8	+0·4				
4371	4406	" " "		42·9	+0·5		42·1	B. G.	−0·3
4789	4823	" June 30		42·8	+0·4		45·2	G. R.	+2·8
4980	5036	" " "		44·0	+1·6		43·9	G. R.	+1·5
5131	5181	" July 5		41·5	−0·9				
5245	5285	" June 30	40·5	40·3	−2·1	41·0	40·8	R. A.	−1·6
"	"	" July 5	40·1			40·6			
5285	5309	" June 30		40·9	−1·5		40·8	A. B.	−1·6
5336	5388	" " "		43·1	+0·7		44·2	B. R.	+1·8
5406	5490	" July 5		41·5	−0·9				
5417	5460	" " 6		40·7	−1·7				
5460	5523	" June 30		43·7	+1·3		42·8	B. G.	+0·4

Number of each star in B. A. C.		Date of observation.	Lat. by B. A. C.		Δ	Lat. by other catalogues.		Name of catalogue.	Δ
				41° 18′		41° 18′			
5449	5604	1859, June 30	43″·4	43″·7	+1·3	43″·6	48″·9	B. G.	+1·5
"	"	" July 5	44″·1			44 ·3			
5525	5560	" " 6		43·8	+1·4		42·6	R. R.	+0·2
5643	5703	" June 30	40·7	41·5	−0·9				
"	"	" July 5	41·9						
"	"	" " 6	41·9						
5703	5752	" June 30		[47·2]	+[4·8]		42·7	B. R.	+0·3
5747	5776	" July 5	40·8	40·2	−2·2	40·5	39·9	G. B.	−2·5
"	"	" " 6	39·5			39·2			
5776	5842	" " 5	41·4	41·9	−0·5				
"	"	" " 6	42·4						
5841	5887	" Aug. 16		40·5	−1·9				
5951	6021	" July 5		42·9	+0·5		42·4	G. A.	0·0
6033	6079	" Aug. 16	[56·1]	[56·7]	+[14·3]	43·8	44·4	R. R.	+2·0
"	"	" " 29	[57·2]			44·9			
"	"	" Sept. 1	[56·7]			44·4			
6150	6185	" Aug. 29		42·3	−0·1		41·9	G. B.	−0·5
6162	6193	" Sept. 1		43·1	+0·7				
6178	6246	" Aug. 16		43·1	+0·7		41·7	G. G.	−0·7
6185	6238	" " 29		41·5	−0·9				
6223	6289	" Sept. 1	40·7	40·8	−1·6	41·5	41·6	G. G.	−0·8
"	"	" " 9	40·9			41·7			
6289	6322	" Aug. 29	43·0	43·5	+1·1	43·2	43·7	G. G.	+1·3
"	"	" Sept. 7	44·1			44·3			
6404	6473	" Aug. 16	41·2	41·2	−1·2				
"	"	" Sept. 9	41·2						
6427	6470	" Aug. 29		43·8	+1·4				
6520	6556	" " 16	43·6	44·4	+2·0	41·9	42·7	R. B.	+0·3
"	"	" " 29	45·4			43·7			
"	"	" Sept. 1	44·3			42·6			
6583	6637	" Aug. 16	42·4	43·6	+1·2	42·3	43·5	G. B.	+1·1
"	"	" Sept. 9	44·8			44·7			
6623	6648	" " 1	41·0	41·7	−0·7	41·9	42·6	G. G.	+0·2
"	"	" " 26	43·2			44·0			
"	"	" Oct. 4	41·0			41·9			
6662	6744	" Sept. 7		43·2	+0·8		43·7	G. B.	+1·3
6674	6712	" Aug. 29	[51·5]	[50·8]	+[8·4]	43·7	43·0	G. R.	+0·6
"	"	" Sept. 9	[50·1]			42·3			
6690	6748	" Aug. 16	42·5	42·5	+0·1	42·0	42·0	G. B.	−0·4
"	"	" Sept. 26	42·5			42·0			
6709	6737	" " 1		41·6	−0·8				
6731	6765	" Oct. 4		44·5	+2·1				
6741	6784	" Aug. 29	[38·5]	[37·8]	−[4·6]	43·8	43·0	R. R.	+0·6
"	"	" Sept. 9	[37·0]			42·3			
6779	6849	" " 26		41·5	−0·9		42·2	G. B.	−0·2
6799	6851	" " 7	42·1	41·6	−0·6				
"	"	" Oct. 7	41·1						
6827	6867	" Aug. 29	43·2	43·1	+0·7	43·4	43·3	B. R.	+0 9
"	"	" Sept. 9	43·1			43·3			
"	"	" Oct. 3	43·4			43·5			
"	"	" " 4	42·8			42·9			
6858	6930	" Sept. 1	42·4	42·5	+0·1				
"	"	" Oct. 6	42·6						
6893	6936	" Sept. 7	42·7	42·7	+0·3	41·2	41·2	G. B.	−1·2
"	"	" " 26	42·6			41·2			
6926	6952	" Aug. 29	41·4	41·3	−1·1	41·6	41·5	G. B.	−0·9
"	"	" Oct. 4	41·2			41·4			
6932	6975	" " 3		42·7	+0·3		42·4	R. B.	0 0
6937	6965	" " 7		45·3	+2·9		43·2	G. G.	+0·8

Number of each star in B. A. C.		Date of observation.	Lat. by B. A. C.		Δ	Lat. by other catalogues.		Name of catalogue.	Δ
			41° 18′			41° 18′			
6965	6997	1859, Sept. 1	42″·6	43″·5	+1·1	42″·2	43″·1	G. B.	+0·7
"	"	" " 9	44·0			43·6			
"	"	" Oct. 7	43·8			43·4			
6959	7029	" Sept. 7		45·4	+3·0				
6963	7022	" " 26		44·4	+2·0		44·6	B. G.	+2·2
6996	7041	" Oct. 3		40·8	-1·6				
7037	7107	" Aug. 29		40·9	-1·5		40·3	B. G.	-2·1
7041	7114	" Oct. 3		39·8	-2·6				
7067	7153	" Sept. 7		40·5	-1·9				
7088	7124	" Oct. 4	40·3	40·1	-2·3	39·8	39·6	G. B.	-2·8
"	"	" " 7	39·9			39·4			
7119	7174	" Sept. 9		40·7	-1·7				
7246	7281	" " 7	42·4	43·0	+0·6	42·5	43·1	B. G.	+0·7
"	"	" " 9	43·6			43·7			
7253	7313	" " 26		42·7	+0·3		42·9	G. B.	+0·5
7377	7437	" " 9	43·1	42·6	+0·2	43·8	43·3	G. B.	+0·9
"	"	" Oct. 4	42·1			42·7			
"	"	" " 7	42·7			43·4			
7437	7476	" Sept. 9	42·0	42·6	+0·2				
"	"	" Oct. 4	42·7						
"	"	" " 7	43·0						
7554	7593	" Sept. 26	40·3	41·2	-1·2				
"	"	" Oct. 3	41·6						
"	"	" " 4	42·5						
"	"	" " 7	40·3						
7607	7642	'58, Nov. 22		40·0	-2·4		40·8	B. R.	-1·6
7627	7683	'59, Sept. 26	41·4	40·7	-1·7	42·7	42·0	A. B.	-0·4
"	"	" Oct. 3	40·3			41·5			
"	"	" " 4	41·1			42·4			
"	"	" " 7	40·0			41·3			
7627	7749	" Sept. 26	40·0	40·0	-2·4	41·5	41·5	A. G.	-0·9
"	"	" Oct. 3	40·1			41·6			
"	"	" " 4	40·3			41·7			
"	"	" " 7	39·8			41·2			
7699	7807	'58, Nov. 22		40·1	-2·3				
7777	7800	" " 18	41·6	42·2	-0·2	41·5	42·1	B. G.	-0·3
"	"	'59, Oct. 4	43·1			43·0			
"	"	" " 7	42·0			41·9			
7843	7888	'58, Nov. 18	[49·0]	[48·1]	+[5·7]	43·8	42·9	R. G.	+0·5
"	"	" " 22	[46·7]			41·5			
"	"	'59, Oct. 4	[48·7]			43·5			
7856	7902	" Sept. 26		41·7	-0·7		42·6	R. G.	+0·2
7961	8032	'58, Nov. 18	43·6	43·5	+1·1	42·7	42·6	B. G.	+0·2
"	"	'59, Oct. 4	43·4			42·5			
7962	8023	" " 18		43·1	+0·7		42·6	G. G.	+0·2
7994	8023	" " 3	43·9	43·5	+1·1	42·9	42·5	B. G.	+0·1
"	"	" " 18	43·1			42·1			
7990	8169	'58, Nov. 8		43·6	+1·2		43·4	G. A.	+1·0
8079	8158	" " 18		44·2	+1·8		42·9	B. R.	+0·5
8177	8238	'59, Oct. 13	43·0	42·3	-0·1	43·3	42·7	G. G.	+0·3
"	"	" " 18	41·6			42·0			
8212	8237	'58, Nov. 18	42·9	42·9	+0·5	43·4	43·4	G. G.	+1·0
"	"	" " 22	42·9			43·4			
8282	8324	" " 18		41·7	-0·7				
8355	8	'59, Dec. 30		45·5	+3·1				
α Cyg.	61¹ Cyg.	" Oct. 3 to Nov. 25, 20 obs.		42·7	+0·3		42·5	A. A.	+0·1
γ Ceph.	ω Pisc.	" Oct. 3 to Dec. 24, 21 obs.		41·8	+0·4		41·7	A. A.	-0·7

If we divide these 92 pairs into four sets of 23 each, as indicated in the table by the lines after Nos. 5336, 6623, and 7119, the mean of each set respectively, and of the whole, will be as follows: column 1, giving the number of the set; col. 2, the number of results of pairs in the set by B. A. C.; col. 3, the mean of these results; col. 4, the number of results of pairs by other catalogues; col. 5, the mean of these; col. 6, the number of results by both B. A. C. and other catalogues taken together; col. 7, the mean of the same; col. 8, the probable error, *E*, of this mean; col. 9, the probable error, *E'*, of a single result for latitude.

	By B. A. C.		By other Catalogues.		By both B. A. C. and others.			
Set	Prs.	Mean.	Prs	Mean.	Num. of results.	Mean Lat.	E.	E'.
1	23	41°18′42″·46	20	41°18″42·′34	43	41°18′42″·40	±0″·15	±0″·97
2	21	42 ·35	14	42 ·60	35	42 ·45	±0 ·14	±0 ·82
3	21	42 ·32	14	42 ·36	35	42 ·34	±0 ·17	±1 ·02
4	22	42 ·35	18	42 ·53	40	42 ·43	±0 ·12	±0 ·76
All	87	41 18 42 ·37	66	41 18 42 ·45	153	41 18 42 ·41	±0 ·07	±0 ·88
	E	± 0 ·10		± 0 ·10		Mean of the whole.		
	E′	± 0 ·97		± 0 ·80				

We may regard 41° 18′ 42″·41, then, as a close approximation to the latitude of the point in New Haven, where the observations were made, the probable error of this result, as given by the observations, being less than a tenth of a second. In taking the arithmetical mean of the results for the several pairs, we assume, of course, for each of these results equal weights—no discrimination having been attempted between errors of observation and of star-places, or between different catalogues. Such discrimination, however, in so large a number of independent pairs, would not have materially changed the result.

It will be seen, from the above table, that any one of the four sets would have given a latitude almost identical with the mean of the whole—in no case differing from it so much as a tenth of a second, or ten feet on the earth's surface. That this extremely close accordance, however, of results from sets composed of so few pairs, is in a measure accidental, may be inferred from the larger probable errors of these results as given in column *E*. Had there been three sets, instead of four, the differences would have been somewhat greater, though still not exceeding a few tenths of a second.

The probable error of a single result for latitude, as given in the last column, (about nine-tenths of a second on an average), includes, it will be understood, the accidental error of observation, (depending on observer, instrument, state of the atmosphere, etc.), together with the error of the stars' places, as given in the catalogues. The experience of the observers of the Coast

Survey with the zenith telescope, as stated by the superintendent, shows that usually the latter error very much exceeds the former, or that the catalogues are less reliable than the instruments. The same is apparent from our own results. While the probable error of a single result (including both error of observation and of catalogue) amounts, on an average to $\pm$ 0″·88, the probable error of observation, as distinct from that of catalogue, is found by comparing the repetitions on the same pairs in our first table, to be only $\pm$0″·48. A similar value of this error is given by a comparison of the longer series of repetitions on the last two pairs of that table (α and 61[1] Cygni, γ Cephei and ω Piscium) as given below. Twenty observations were taken of the former pair, and twenty-one of the latter. The star-places used are those of the English Nautical Almanac.

Date of Obs.	α Cygni and 61[1] Cygni.	Δ	Date of Obs.	γ Cephei and ω Piscium.	Δ
1859.			'58, Nov. 8	41° 18′ 41″·2	−0″·5
Oct. 3	41° 18′ 42″·9	+0″·4	11	41 ·4	−0 ·3
4	42 ·0	−0 ·5	'59, Oct. 15	42 ·4	+0 ·7
7	41 ·9	−0 ·7	18	42 ·1	+0 ·4
10	41 ·9	−0 ·7	21	41 ·3	−0 ·4
11	43 ·7	+1 ·2	27	43 ·2	+1 ·5
15	41 ·8	−0 ·8	29	40 ·8	−0 ·9
18	42 ·7	+0 ·2	Nov. 1	42 ·0	+0 ·3
21	42 ·3	−0 ·2	3	41 ·4	−1 ·3
23	42 ·9	+0 ·3	14	43 ·3	+1 ·6
25	42 ·2	−0 ·4	15	40 ·6	+1 ·1
28	43 ·6	+1 ·0	23	42 ·4	+0 ·6
29	42 ·8	+0 ·2	28	41 ·7	0 ·0
Nov. 2	42 ·8	+0 ·2	Dec. 8	43 ·0	+1 ·3
3	43 ·4	+0 ·9	10	40 ·2	−1 ·5
4	43 ·0	+0 ·5	12	41 ·6	−0 ·1
9	43 ·2	+0 ·7	15	41 ·0	−0 ·7
14	43 ·0	+0 ·4	16	41 ·9	+0 ·2
17	41 ·7	−0 ·9	21	41 ·2	−0 ·5
19	42 ·0	−0 ·6	23	41 ·6	−0 ·1
25	41 ·3	−1 ·2	24	41 ·9	+0 ·2
Mean	41 18 42·55	±0″·10	Mean	41 18 41·73	±0″·12
Prob. Er. of single result		± 0 ·47			±0 ·55

We have, then, for the probable accidental error of *observation*, from the repetitions on the same pairs in our first table, $\pm$0″·48; from α and 61[1] Cygni, $\pm$0″·47; from γ Cephei and ω Piscium, 0″·55; from all these combined, $\pm$0″·50. This compared with the average probable error of a single result for latitude, $\pm$0″·88, in which it is included, gives for the probable error depending on the catalogues used, $\pm$0″·72. The relative accuracy, therefore, of instrument and catalogues, in the present case, may be represented approximately by the ratio of 5 to 7. That the Twelve Year and other catalogues used are, on the whole, more accurate than that of the British Association, appears from the values of E' in the last line of the table on page 58; viz. $\pm$0″·97 for B. A. C., and $\pm$0″·80 for the other catalogues; although in the column for

the latter, as may be noticed, many of the positions for one of the stars of a pair, are also from B. A. C. If we separate from these values the probable error of observation, ±0″·50, before stated, we shall have for error depending on the catalogues, ±0″·82 for B. A. C., and ±0″·63 for the others. This result accords well with the remark of Prof. Bache, in a paper published in the 12th volume of the Proceedings of the American Association, that "the probable error of the result by a single pair of stars as depending on the catalogue errors of the stars' places, will range from ±0·″60 to ±1·″00."

In the same paper it is stated also, that "a probable error in observing of from ±0″·25 to ±0″·80 may be expected for one observation, according to the size and quality of the instrument and the ability of the observer." The average value of this error for some 300 pairs of stars, observed with the excellent instruments and by the experienced observers of the Coast Survey, is, according to the same paper, ±0″·47—ranging from ±0″·25 to ±1″·14. The value of the corresponding quantity in our own results, is, as we have seen, ±0″·50, ranging from ±0″·47 to ±0″·55. A considerable portion of this is due, doubtless, to the want of sufficient sensitiveness in the alidade level of the instrument; the run of which, as before stated, is only one thirteenth of an inch for 5″, smaller portions of the scale than this being measured by the eye. This level, we may remark, was intended originally only to be used in the ordinary way, in connection with the graduated circle, and is sufficiently sensitive for that purpose; but not enough so to correspond, in point of accuracy, with the micrometer,* or for the delicacy required in observations for latitude with the zenith telescope. The use of the instrument for such observations was wholly an after thought, and no change was made to adapt it to its new functions. It is of smaller size also than the zenith telescopes of the Coast Survey, and it may be proper to add, that, being of the observer's own make, and a first attempt of the kind, some allowance is due in a comparison of its results with those of more finished instruments in the hands of trained observers.

In order to compare the latitude, thus determined astronomically, with the geodetical determination of the Coast Survey, a survey was made with theodolite and chain connecting the writer's observatory with three points in the city, the positions of which are given in the Coast Survey Report for 1851, viz., "Brewster's Factory Cupola" at the foot of Wooster street, "Episcopal church," which means the west spire of St. Paul's,

* Sixty successive bisections of Polaris, near culmination, with the micrometer wire, gave for probable error of a single bisection, ±0″·30; which would make the probable error of a single result for latitude (involving two bisections) as depending on the micrometer alone, ±0″·21.

and "College Spire," which is that of the Lyceum, or middle spire. The results are as follows.

	Lat. by Talcott's method.	By Coast Survey.	Diff.
Observatory of the writer, ...	41° 18′ 42″·41		
Factory Cupola, East Street, ...	3·29	41° 18′ 7″·10	3″·81
St, Paul's Church, W. Spire, ...	7·52	11·32	3·80
College Observatory, (Athen. Tower),...	22.36		
"College Spire," (Lyceum),............	23·90	27·74	3·84
Chapel Spire, ...	25·49		

Mean excess of geodetic over astronomical lat. 3″·82.

It may be added that a series of observations made in 1850, with an excellent Pistor and Martins' Patent Sextant, on northern and southern stars, and also a series of observations made in 1858, in the usual way, with the circle of the Transit instrument before described, on Polaris and other stars, both direct and by reflection, and in reversed positions of the instrument, gave results differing but about half a second from the above determinations, the former minus, the latter plus.

The differences among results for latitude, depending on differences of catalogues, are often considerable, even when the best authorities only are trusted; as will be seen by comparing the mean results, given below, of the observations on α and 61^1 Cygni, and γ Cephei and ω Piscium, according as the declinations are taken from the several catalogues named.

	α and 61^1 Cygni.	γ Cephei, ω Piscium.
English Nautical Almanac,............	41° 18′ 42″·5	41° 18′ 41″·7
Twelve Year Catalogue,..............	42·6	41·4
B. A. C.,...	42·7	41·3
Greenwich, 1850,....................	42·8	41·6
Washington, 1845–1850,..............	42·7	
Radcliffe, 1856, '57,................	41.9	40·3
Bradley,...........................		40·7
Piazzi, ...		41·1
Argelander,........................		41·8
Mädler, ...		41·4

It will be noticed, also, in respect to these two pairs, that there is a greater difference between their respective mean results, than we should expect from well determined nautical almanac stars. This difference between the means of the twenty and twenty one observations respectively on the two pairs, using the nautical almanac declinations, amounts to 0″·8, an amount difficult to attribute to any errors of instrument or of observation, and equally difficult, perhaps, to ascribe to errors of stars' places, especially as the several catalogues so well agree with each other. From whatever source it may arise, it seems to be constant, and will receive further investigation.

ART. VIII.—*On Fixing Magnetic Phantoms*; by Prof. J. NICKLÈS.

THE name phantom was given by M. de Haldat* to the figures which are obtained when iron filings are thrown upon a sheet of paper or a pane of glass placed over a magnet. This physicist fixed these images by producing them upon a sheet of paper coated with starch or prepared with gelatine.

This process certainly enables us to obtain the general form of the phantoms, but all physicists can see that it suppresses the details. I was more particularly struck with this fact on a recent occasion, where I sought to fix the phantoms of some new electro-magnetic combinations; I therefore propose another method, which is here briefly given; it is very simple and succeeds perfectly. The paper upon which the phantoms are to be fixed is "waxed" paper. A sheet of this is placed over the poles of the magnet in question, and kept in a horizontal position by means of a screen placed between the paper and the magnet. Then proceeding in the usual manner, when the image is fully developed, a hot brick is held above it, or the warm lid of a crucible, which is preferable because it is lighter, and easily managed with the tongs. They must not touch the paper, but only be brought within the distance necessary to fuse the wax. As soon as this happens, which is easily perceived by the glistening appearance produced, the brick is withdrawn. Meanwhile the current does not cease its activity, nor the filings lose their arrangement, in which position the whole solidifies so well that the fixed image does not at all differ from the phantom of the magnet in activity.

This result is explained as follows. By capillarity the melted wax penetrates the masses of filings, very much as water penetrates a heap of sand; the heat of the brick facilitates this, by preventing the solidification of the wax, and as the temperature is not sufficiently elevated to sensibly affect the magnetism developed, the phantom, after the solidification, and in the most minute details, preserves the same arrangement which the iron filings had, while they were free to obey the action of the magnet.

A condition indispensable to success, is, that the stratum of wax has a sensible thickness, so that it may suffice for the agglomerations, since these absorb melted fatty matter, even to saturation. That this force of absorption is very energetically exercised, may be perceived after the cooling, since the paper about the agglomerations is deprived of wax, and differs thus in appearance from those parts where capillarity has not been exercised. It is therefore possible to preserve to the phantoms the

* Memoir before the Academy of Stanislas, p. 48, for the year 1839—1840.

relief which has before been sought in vain, and what will be still more useful, to give permanence to the sort of molecular arrangement which the filings take, when exposed to magnetic influence.

Instruction can hardly fail to be derived from the use of these means, by aid of which it will be possible to study the figures more advantageously, which are, in some sense, the visible expression of the force animating bodies endued with polarity developed by magnetism.*

Nancy, March, 1860.

ART. IX.—*On some Questions concerning the Coal Formations of North America;* by LEO LESQUEREUX. Continued from Vol. xxviii, p. 21.

Geographical Distribution of the Coal Flora.

To follow the plan exposed in the first part of this memoir (Silliman's Journal, No. 82, July, 1859, p. 21) I should have to examine now the nature of the coal-flora, that is, the anatomical and chemical constitution of the coal plants, first comparing between themselves the groups of species of plants from which the matter of the coal is a compound, and then examining how this vegetation is related to the plants living at this epoch. The distribution of the coal plants, either geographical or stratigraphical, is a question accessory to the former. Nevertheless, it has become now of a greater importance, since it touches upon a problem which is at present discussed by the authority of the highest scientific names. I allude to the theory *of the origin of species* by Mr. Darwin. It evidently concerns the great problem of the inmost nature of man, and thus forces every naturalist to seek, in the sum of facts gathered up by his researches, either confirmatory or contradictory evidence of views which cannot but preoccupy his mind. Thus it is apparently advisable to change the order of examination of the flora of the coal measures of North America, studying it now in its stratigraphical and geographical distribution, and leaving for another opportunity the discussion concerning the nature of its vegetation and the specific and generic value of its representatives.

* This note is extracted from a work now in press and which will soon appear, entitled, *Electro-magnets and Magnetic adhesion*, and accordingly treats of electro-magnets and their application to locomotion upon railways, with the design of increasing the adhesion of locomotive engines. To the experiments which we made public in 1853 (v. this Jour., [2], vol. xvi, p. 337), we shall add new ones which allow us to expect a speedy solution of this problem, so important to railroads.

In regard to electro-magnets, we will only say at present, that we shall mention a great number of new species, which have compelled us to invent a new classification and a nomenclature based upon the principles of the natural method. We have also to make known a great number of new facts respecting the laws and properties of electro-magnets, to say nothing of what we wrote upon this point in 1853 (v. Am. Jour., [2] vol. xv, p. 381, and vol. xx, p. 100). J. N.

I shall not attempt in any way either an exposition or a critical examination of the views of the celebrated English author. This task has already been admirably fulfilled in a former number of this Journal.* I shall merely expose the facts that appear surely ascertained by a long and careful exploration of the coal-fields of North America, leaving the naturalist-philosopher to take from these facts any conclusion that may appear just to him. It is a mite only. But the monuments of humanity, like the mountains of limestone, are built by the slow accumulation of minute remains.

The botanical palæontology of the coal-period and the succession and variation of species in the different strata of the coal-measures, cannot be studied with more advantage and with more chances of reliability than in the coal-fields of the United States. Their immense area, the uniformity of their generally unbroken stratification, the facility of ascertaining in many localities the order of this stratification, the numerous exposures of coal banks, not limited to a particular district, but opened and worked now at distant points over the whole area of the coal-fields; all this affords to a systematic exploration such advantages as cannot be found in any other country of the world.

Moreover, my explorations of the coal-fields of North America have been favored by peculiar circumstances. Connected at different times, during ten years, with the geological surveys of the states of Pennsylvania, Kentucky, Indiana, Arkansas and Illinois; called to survey for comparison the coal-fields of Ohio and part of those of Virginia; constantly limiting my researches to botanical palæontology, I have thus, I suppose, collected on the distribution of the coal-plants in North America, more materials and more reliable accounts than geologists may be able to get at for a long time to come.

The first important question in regard to the coal-plants of America is: what is their relation of forms with the plants of the same formation in Europe? The comparison of the coal flora of both continents has never been made except on few and insufficient data.†

Except two, all the genera of fossil plants of the coal-fields of America are represented in Europe. One of them is the remarkable *Whittleseya elegans* of Dr. Newbury, a flabellate, apparently short pedicillate, cuneiform-oval and truncate leaf, found hitherto always detached from the stem. It evidently differs from the genus *Cyclopteris* by its simple straight nervation and by its up-

* Vol. xxix, p. 1.

† I have attempted it formerly, in my palæontological report of the Geological State Survey of Penn., delivered January 1st, 1854, but only published five years afterwards. As I have been denied the privilege of reviewing and correcting my manuscript before its publication, I cannot consider myself accountable for the errors, and especially for the deficiency of conclusive data, which may be found in it.

per margin being horizontally truncate and regularly wavy-denticulate by the percurrent and slightly emerging nerves. The typical affinity of this plant is unknown. It is perhaps more related to *Cordaites* or even to *Salisburia* than to a fern.

The second genus peculiar to the American coal-flora is my *Scolopendrites*, represented, like the former, by a single species; *Scolopendrites dentata* Lsqx., of which fragments only have been found. The name has no relation to the nervation of the leaf, but to its outline. This leaf is apparently five to six inches long, more than an inch broad, lanceolate, deeply cut by obtuse somewhat regular teeth and marked by a few very thin distant nervules, emerging from a narrow medial nerve in an acute angle, scarcely arched and forking twice. Both these genera represent peculiar types to which no form of the European coal-flora can be compared. I could also mention as a peculiar type of our coal-flora *Crematopteris Pennsylvanica* Lsqx., a thick stem or branch, on both sides of which short, lanceolate, obtuse, thick leaflets, without any trace of nervation, are pinnately attached. The single specimen which I have found of this plant was not well preserved enough, and it would be unsafe to consider the species which it represents as having been seen in its true form and full development.

If we admit the generic distribution of the fossil plants of the coal, as it has been established by Brongniart in his *Tableau des Genres,* (certainly the best that has been attempted either before or after him) all the European genera, even the undefined genus *Aphlebia,* (Sterbg.) have representative species in the coal-fields of America. From the nomenclature of Göppert and Corda, a few European genera, it is true, are hitherto without representative species in our coal-fields. But these genera, established on the form and the position of fructification, always very difficult to identify from even the best preserved specimens of fossil ferns, may be represented by some species of our *Sphenopterideæ* and *Pecopterideæ.* These genera, viz., *Rhodea* Sternb., *Trichomanites* Göpp., *Steffensia* Göpp., *Beinertia* Göpp., *Diplaxites* Göpp., *Woodwartites* Göpp., are no peculiar types. Thus, considering their generic distribution, the coal-plants of Europe and of North America show very little difference indeed. But in examining the species separately and comparing them on both sides of the Atlantic, the number of forms peculiar to America appears much larger than it was at first supposed, and thus the vegetation of our continent, at the epoch of the coal formation and considered in its whole, is far more different than it has been supposed heretofore.

It would be too long and tedious, perhaps, to take one by one and compare all the American species with those of Europe,

mentioning the identity, the relation, or the difference of each of them. I will therefore give in a table the number of species of each genus, belonging either to North America or to Europe, or common to both continents, and complete this general view by a few remarks on some of the species, apparently the most interesting, by predominance of number, of typical form or by a peculiar distribution.

Genera of Coal plants.	Species peculiar to America.*	Species peculiar to Europe.	Species common to both.
1. Noeggerathia Sternb.,	3 + 2*	5	1
2. Cyclopteris Brgt.,	1	2	2
3. Nephropteris Brgt.,	5	4	4
5. Neuropteris Brgt.,	18 + 1*	16	12
6. Odontopteris Brgt.,	4 + 1*	6	3
7. Dictyopteris Gutb.,	1	1	0
8. Sphenopteris Brgt.,	10 + 9*	41	12
9. Hymenophyllites Göpp.,	6	10	2
10. Rhodea Sternb.,	0	1	0
11. Trichomanites Göpp.,	0	4	0
12. Steffensia Göpp.,	0	1	0
13. Beinertia Göpp.,	0	1	0
13. Diplaxites Göpp.,	0	2	0
14. Woodwartites Göpp.,	0	2	0
15. Alethopteris Sternb.,	9 + 2*	20	9
16. Callipteris Brgt.,	2	1	1
17. Tecopteris Brgt.,	12 + 4*	49	12
18. Aphlebia Sternb.,	0	6	1
19. Caulopteris Brgt.,	4	4	0
20. Psaronius Brgt.,	10	6	0
21. Crematopteris Schp.,	1	0	0
22. Scolopendrites Lsqx.,	1	0	0
23. Whittleseya Newb.,	1	0	0
24. Cordaites Ung.,	1	0	2
25. Diplothegium Corda,	0	0	1
26. Stigmaria Brgt.,	5	2	5
27. Sigillaria Brgt.,	12 + 9*	37	17
28. Syrigodendron Brgt.,	0 + 1*	0	2
29. Diploxylon Corda,	0	0	1
30. Lepidodendron Brgt.,	14	10	11
31. Ulodendron Rhode,	0	4	2
32. Megaphytum Artis,	1 + 1*	4	0
33. Knorria Sternb.,	2 + 2*	1	2
34. Halonia Ll. & Hutt.,	0	2	1
35. Lepidophyllum Brgt.,	7	2	4
36. Lepidostrobus Brgt.,	?	1	2

* In this enumeration, I count the species named as new ones in a catalogue published in 1853 by Dr. Newberry (Nos. 8th and 9th of the Annals of Science of Cleveland). As the species have not been described and did not come under my examination, they are still doubtful and separately marked by a *.

Genera of Coal plants.	Species peculiar to America.	Species peculiar to Europe.	Species common to both.
37. Cardiocarpon Brgt.,	2 + 5*	6	0
38. Trigonocarpum Brgt.,	3 + 3*	5	5
39. Rhabdocarpos Göpp. & Brgt.,	1 + 1*	6	1
40. Carpolithes Sternb.,	12 + 1*	52	6
41. Selaginites Brgt.,	0	1	0
42. Lycopodites Brgt.,	1	12	0
43. Lomatophloios Corda,	1	0	1
44. Lepidophloios Sternb.,	0	0	1
45. Bothrodendron Göpp.,	0	1	0
46. Cycadloidea Buckl.,	0	1	0
47. Calamites Suck.,	2	5	11
48. Bornia Sternb. & Göpp.,	1	1	0
49. Equisetites Sternb.,	0	2	1
50. Asterophyllites Brgt.,	5	8	7
51. Annularia Sternb.,	0	0	5
52. Sphenophyllum Brgt.,	2 + 3*	3	3

Noeggerathia Sternb.—Two of the American species are closely related to *N. obliqua* Göpp. A third, *N. Bocksiana* Lsqx., which I referred to *Cyclopteris Bocksiana* Göpp., from the exact likeness of its terminal leaflet with the separate and only leaf on which the author has established his species, is evidently referable to the genus *Adiantites* Göpp. The two species of true *Noeggerathia* belong to the Old Red Sandstone of Pennsylvania; the third found at the base of the millstone grit of Pennsylvania belongs by its typical form to the true coal measures.

Nephropteris Brgt.—Cannot be separated from the genus *Neuropteris*, since it represents large, mostly rounded and deciduous leaflets attached around the stem at the base of secondary pinnæ of some *Neuropteris*. Two of our American species, viz., *Nephropteris fimbriata* Lsqx. and *Nephropteris laciniata* Lsqx. have a typical character which has never been seen on any of the fossil ferns of Europe. As the names indicate, both these beautiful species of leaves are fringed and laciniate on their circumference. The fringes and laciniæ are unequal in length and breadth, flexuous, and do not bear any likeness to the straight and regular points surrounding the fruit-bearing leaves of some ferns of our time.

Neuropteris Brgt.—Our *Neuropteris hirsuta* Lsqx. is, probably at least, the equivalent of *Neuropteris cordata*, *N. Scheuxeri*, *N. angustifolia* and *N. acutifolia*, species of M. Brongniart. A peculiar character of this species, viz., the long, straight, diffuse hairs of its upper surface may help to bring together the numerous and diverse forms of its leaflets, which are generally separated from the stem. Mr. Bunbury had already remarked and mentioned those hairs. I have not been able to detect them on the few, badly preserved specimens of the European *Neuropteris cor-*

data Brgt. which I had opportunity to examine. It may be also that *Neuropteris smilacifolia* Sternb., *N. plicata* Sternb., *N. rotundifolia* Brgt., ought to be referred as varieties to *Neuropteris flexuosa* Brgt. Nevertheless, we have these same scarcely distinct species with just the same characters in America. They are mostly found in the same places where *Neuropteris flexuosa* abounds. Of the other American species of this genus, *Neuropteris Clarksoni* Lsqx. is closely allied to *Neuropteris auriculata* Brgt., and *Neuropteris Desorii* Lsqx., to *Odontopteris Reichiana* Gutb. But *Neuropteris speciosa* Lsqx., *N. rarinervis* Bunby., *N. gibbosa* Lsqx., *N. undans* Lsqx., *N. dentata* Lsqx., *N. Morii* Lsqx. are widely different from any European species, and may be considered as true American forms or types. *Neuropteris adiantites* Lsqx. should perhaps be referred, by its peculiar nervation, to the genus *Sonopteris* of M. Ponsel.

Odontopteris Brgt.—I have counted *Odontopteris Brardii* Brgt., among the species belonging to both continents, on the authority of M. Unger, who indicates it at Mauch-Chunk, Pennsylvania. Nevertheless I doubt of the identity of our species with the true *O. Brardii* Brgt. The small specimen which I formerly referred to it belongs to another species, *Odontopteris crenulata* Brgt., and I would rather suppose that some incomplete specimen of *Odontopteris alata* Lsqx., has been mistaken for the true *O. Brardii* Brgt. Our species, though closely related to the European form, evidently differs from it by its obtuse leaflets, and by the position of two large, opposite, cuneate, truncate leaflets attached to the rachis just below the base of the pinnæ.—*Odontopteris Schlotheimii* Brgt., common to both continents, is one of the few species which, either in its fructified or sterile form, show a perfect identity at any locality where it is found.

Dictyopteris Gutb.—The affinity of the European *Dictyopteris Brongnarti* Gutb. with our *Dictyopteris obliqua* Bunby., is so great that these species could be considered as mere varieties of the same. *D. obliqua* Bunby. has narrower, less obtuse leaflets. These are generally found detached from the stem and spread over the shales in the greatest abundance.

Sphenopteris Brgt.—This genus, to which I have united the *Gleichenites* of M. Göppert, appears from the number of its species, to be far more abundantly represented in Europe than in America. Probably the difference may be accounted for, first by the number of European species which, established on small specimens, represent variable parts of a frond and may be recalled to a common species. Secondly by the insufficiency of our explorations. Since the publication of my catalogue of fossil plants of the coal, in 1858, ten new species of *Sphenopteris* not counted in the table, because they are still undescribed, have come under my examination. From the number of species enu-

merated by Dr. Newberry it would appear also that this genus is largely represented at some places and in restricted areas. Thus we may expect to see the number of American species largely increased.

The essential European types of this genus are all represented in our coal measures, either by identical species, or, by closely related forms. Thus we have: *Sphenopteris spinosa* Ll. and Hutt., *S. tridactylites* Brgt., *S. Davalliana* Göpp., *Sphenopteris irregularis* Gutb., *S. Gravenhorstii* Brgt., *S. Dubuissonis* Brgt., *S. latifolia* Brgt., *S. polyphylla* Ll. and Hutt., *S. artemisiæfolia* Sternb., &c. Besides, as American types, we have *Sphenopteris flagellaris* Lsqx., and especially *Sp. Newberrei* Lsqx. This last species has a ramification different from that of any other kind of fossil fern, viz., two secondary pinnæ, joined at their base and forking in an obtuse angle at the top of a short naked stem. The form of the leaflet and the nervation of this species is also peculiar. One of my species, *Sphenopteris decipiens* Lsqx. has, like *Neuropteris adiantites* Lsqx., the primary nerve folded along the base of the leaflets and the nervules curved and branching upwards, a character ascribed by Brongniart to the genus *Lonopteris* Tom.

Hymenophyllites Göpp.—From analogy of nervation, rather than from a relation of typical form, I have, perhaps wrongly, connected with this genus, some species referable either to *Aphlebia* Sternb., or to *Schizopteris* Brgt., or even to *Selaginites* Brgt. Four American species: *Hymenophyllites fimbriatus* Lsqx., *H. affinis* Lsqx., *H. hirsutus* Lsqx., *H. laceratus* Lsqx., are represented by fronds which form a broad base, divide in ascending, and are thus irregularly cut in simple, ordinarily short, curved, somewhat obtuse laciniæ or lobes. Each of the divisions is marked by a single nerve, ascending to its top. The fronds appear generally of a thick texture; but in *H. giganteus* Lsqx., which may be the same plant as *Schizopteris lactuca* Sternb., they are seemingly very thin. These species ought to constitute a separate genus. In the fossil flora of Pennsylvania I had attempted to group them together under the name of *Pachyphyllum;* but as some species have apparently thin leaves or fronds, the name could not be preserved of course. A discussion concerning the morphology of the plants of the coal would be out of place now. The subject ought to be separately treated. I needed only to mention these peculiar forms, for comparing the distribution of the genus *Hymenophyllites*. Two of our American species are closely related to the *H. elegans* Brgt. of Europe. The others, especially *H. fimbriatus* Lsqx., are apparently peculiar American types. This last, nevertheless, could be compared to *Selaginites Erdmani* Gutb.

Alethopteris Sternb.—All our American species have some affinity with European types of the same genus. Even the re-

markable *Alethopteris serrula* Lsqx. is related, though distantly, to *A. erosa* Gutb., from which it differs, especially by its very long (sometimes 4 to 5 inches), narrow, linear pinnæ. It has been found only near Pottsville, Pennsylvania.

Callipteris Brgt.—One of the American species, *C. Sullivantii* Lsqx., is a beautiful fern of a peculiar character, apparently bi- or tri-pinnate, but of which pinnæ only have been found. The general outline of the pinnæ is like that of *Alethopteris Serlii* Brgt. But the pinnules are twice as broad, rounded at the top, marked by a broad and deep medial nerve, abruptly terminating in the middle of the leaflets, and from which emerge in acute angles, thin arched, forked nervules as closely placed as those of *Neuropteris flexuosa* Brgt. The other American species is closely related to *Alethopteris sinuata* Brgt. *A. nervosa* Brgt. is a common species over the whole extent of our coal fields. As it is generally found connected (though not on the same stem) with *A. Sauveurei* Göpp. (*A. nervosa* var. Brgt.), both forms are referable to the same species.

Pecopteris Brgt.—The identification of the species of this genus is difficult. Here, as with the *Sphenopterideæ*, some species have been established on fructifications, which are seldom found in such a state of preservation that the form and the position of the fruit-dots can be ascertained, and others have been differently named from the branching of the veinlets, which differs in the same species according to the position of the leaflets. Thus identical species have received different names or even been placed in different genera. The number marked in the table represents only species of ours of which the value is ascertained. Some of them show apparently true American types. Thus, *Pecopteris Sillimani* Brgt., *P. Loschii* Brgt., *P. Velutina* Lsqx., *P. distans* Lsqx., *P. decurrens* Lsqx., and *Pecopteris concinna* Lsqx., widely differ from any European fossil *Pecopterideæ* known till now.

Caulopteris Brgt.—Uniting *Stemmatopteris* Corda, to this genus, we have at least four distinct species.

Stigmaria.—I think that the varieties of *Stigmaria anabathra* Corda, described by M. Göppert, are true species, being generally found in our coal-fields at different geological horizons. The five species described as new in the report of the Pennsylvania survey, may be perhaps reduced to three, but some well characterized species have been found since the report was made.

Sigillaria Brgt.—The distribution of this genus does not operate identically on the coal-fields of both continents. We have few species of the narrow-costate *Sigillariæ*, more than 30 species of which are counted in Europe. We have more of the broad-costate forms and especially a large number of species belonging to the peculiar section of the smooth or rather uncostate *Sigillariæ*.

In this last section, three well characterized forms are peculiar to our coal-fields. The one has its surface stellately wrinkled around the scars; the second is wrinkled crosswise, and the third has double, oval, obliquely-placed scars united by a deep groove, and the surface is beautifully reticulated by narrow wrinkles, obliquely intersecting each other. The beautiful *Sigillaria Schimperi* Lsqx., of which the large scars have nearly the form of an eye, is also a peculiar American form. The number of our species, as marked on the table, is too small. About ten new species belonging to the Geological State surveys of Kentucky, of Arkansas and of Illinois are not here counted.

Syringodendron Brgt.—The two species described by Mr. Brongniart are common in America. I have never seen any other. Dr. Newberry indicates a peculiar species of ours under the name of *S. Americanum* Newb.

Lepidodendron Sternb.—The great number of specimens of this genus collected from the base and the top of the millstone-grit series of our coal measures, has afforded a good opportunity for examining the development and variations of the scars at the different stages of growth of the trees. Except *L. oculatum* Lsqx., which might be referred perhaps to *L. distans* Lsqx., all the new species described in the report of the survey of Pennsylvania are well characterized. Some of them might be considered as American types. *L. distans* Lsqx., is related, by the distance of the scars only, to *Sagenaria rimosa* Sternb.

Megaphytum Artis.—A beautiful and large specimen of a new species of this genus is preserved in the cabinet of the Geological State Survey of Illinois. Another new species is mentioned by Dr. Newberry under the name of *M. discretum* Newb.

Knorria Sternb.—The number of American species is apparently pretty large; but the difficulty of determining the species from specimens generally badly preserved has prevented or retarded the descriptions. *Knorria imbricata* Sternb., is especially common below the millstone grit.

Lepidophyllum Brgt.—We have already seven well characterized American species, and three new and unpublished ones. The number of peculiar forms of these scales or leaves of the cones of *Lepidodendron*, evidently shows that the large proportion of species of *Lepidodendron*, which have been found in America, cannot be considered as resulting from peculiar changes of the same species, according to the age of the scars. *Lepidophyllum hastatum* Lsqx., *L. brevifolium* Lsqx., and *L. plicatum* Lsqx., are American types not related to any European species.

Lepidostrobus Brgt.—The number of cones of *Lepidodendron* is extremely large, especially in the shales of the first bed of coal above the conglomerate. All the species ascertained, from the

form of their scales, apparently the only reliable specific character for a determination, are counted with *Lepidophyllum.*

The fruits distributed among the genera *Cardiocarpum* Brgt., *Rhabdocarpos* Göpp. and Berg., *Trigonocarpum* Brgt., *Carpolithes* Sternb., are generally found broken or flattened or divested of their outer envelope. Consequently the identification of the species is very difficult. The number of these fruits disseminated in the shales and in the sandstone of the low coal of our American basin is considerable. They follow the stratigraphical distribution of the genus *Lepidodendron,* though they do not appear in any way related to this genus. About twenty well marked but undescribed species ought to be added to those counted in the table. Some of them have peculiar forms without relation to any European species. One of the most remarkable, *Rhabdocarpos arcuatus* Lsqx., is described and figured for the fourth volume of the Geological Report of the State Survey of Kentucky, under the direction of Dr. D. Dale Owen.

Lycopodites Brgt.—Nothing, perhaps, shows more evidently the difference in the characters of the coal flora of both continents than the scarcity of species of *Lycopodites* and the abundance of species of *Lepidodendron,* in our coal measures; when a contrary distribution rather predominates in Europe. Both the genera are considered as closely related. Nevertheless we have a single species of *Lycopodites,* very rare indeed, since I found specimens of it only lately on both the extreme limits of the state of Kentucky, at the same geological horizon, viz., in the shale of coal No. 1B. By its concave leaves, decurrent and embracing at the base, it differs from any of the twelve European species of the coal yet described Dr. Newberry has not mentioned any species of this genus in his catalogue.

Asterophyllites Brgt.—As some species of this genus are represented by catkins, scarcely if ever found attached to the stems, and of which therefore the relation is obscure or unknown, I may have counted as peculiar to America a few species which are only fruiting modifications of some others. I consider those fruits as male catkins, attached to large branches, while the true fruit-bearing catkins, which are much smaller, were apparently terminating the branchlets;—a kind of fructification somewhat analogous, but in a position contrary to that of some pines.

Without including the species of Dr. Newberry, the table of distribution shows that from six hundred and fifty-five species of coal plants now determined; more than one hundred and sixty are peculiar to America; three hundred and fifty species are known only in Europe; and one hundred and fifty are common to both. It is certain that future researches will greatly add to the number of species common to both continents, but as much, also, to the number of species peculiar to America. There-

fore, the difference pointed out by the table, may be admitted as fairly representing, in a proportional manner, the general distribution of the coal plants on both continents. The botanical remains of the coal-fields of Europe have been carefully collected and studied by learned naturalists for more than a century, while those of America are only beginning to be noticed by scientific explorers.

In the Introduction to the Fossil Flora of the coal-fields of Pennsylvania, I had already pointed out the great analogy existing between the plants now living on the peat-bogs of America and of Europe. Admitting the peat-formation of our time as being the actual representative of the coal-marshes of the coal-epoch, I was led to the conclusion: that at this last epoch, the flora of both continents was as different and even more different than its representative flora is at our time. Thus, on twenty-five species of mosses growing on the peat-bogs and entering into the formation and composition of the peat, a single one is peculiar to North America. By extending my researches to the South, namely to the latitude of Norfolk, in the great Dismal Swamp of Virginia, I found the proportion changed in some manner; but nevertheless, the most common forms of the genus *Sphagnum*, which in Europe and North America form the principal mass of the peat, were found there also, performing the same work in the composition of the combustible matter. In the family of the Ferns, out of ten species growing on our marshes, five are identical with European species growing in the same situations; and two more are so closely allied to their European congeners that in a state of petrifaction, they could not be distinguished from each other. Even now, in their normal state, they are admitted by some botanists as varieties only. Among the *Junceæ*, *Cyperaceæ* and *Gramineæ*, twenty-six species out of forty-one are common to both continents; and from the other families, of which representatives are found on the peat-bogs, twenty-six species of thirty-one are found in Europe and in America. The likeness of some species of this section, peculiar to both continents, is still remarkable. Thus, *Larix Americana* and *Larix Europæa*; *Nymphæa odorata* and *Nymphæa alba; Ledum latifolium* and *Ledum palustre*, *Trientalis Americana* and *Trientalis Europæa; Vaccinium macrocarpum* and *V. Oxycoccos* (many others still could be named), are so nearly related that their specific characters can be distingnished only on good and complete specimens. There are, indeed, on the peat-bogs of America, some peculiar types which are not found in Europe: *Xyris bulbosa*, *Taxodium distichium*, *Sarracenia purpurea*, and a few others. But it is even so with the plants of the coal-period where we have seen certain types peculiar to this continent. This peculiarity serves only to render the more strik-

ing the analogy of distribution of the flora of both epochs. It shows the same degree of difference and of analogy. Some species, even a few types, peculiar to each country, the greatest number of them peculiar to America; many identical species, and especially many forms, so nearly related, that it becomes very difficult to separate them by specific characters.

Columbus, Ohio, April, 1860.

(To be continued.)

ART. X.—*On an Oil-Coal found near Pictou, Nova Scotia; and the Comparative Composition of the Minerals often included in the term Coals;* by HENRY HOW, Prof. Chemistry and Nat. Hist., King's College, Windsor, N. S.

THE name given to the substance I purpose describing indicates the use to which it is put, viz., the manufacture of paraffine-oil, and an inquiry into the association of elements in the minerals constituting the sources of this and similar "mineral-oils" and in the bituminous coals, may possess some interest in a chemical point of view. As regards the classification of these it is not necessary to do more than recall the attempt made a few years ago in courts of law, in Scotland, New Brunswick and Nova Scotia, to decide what should and what should not be called a coal. The great array of evidence of various kinds brought to bear on the question rendered it a very interesting one, and it is well known that the opinions of the numerous scientific men consulted on these occasions were so nearly balanced that the point at issue was determined on the commercial, rather than on the scientific merits of the cases. It will be remembered that the substances in dispute were the Torbane-hill "coal," found near Bathgate in Linlithgowshire, Scotland, and the Albert "coal," occurring at Hillsborough, New Brunswick. As respects the former, the result of the trial in Edinburgh, in 1853, was that the jury considered it to be "coal, in the common sense of that word;" and, as regards the latter, it was decided at Fredericton, N. B., 1852, and at Halifax, N. S., to be also a "coal." Notwithstanding these legal decisions, which, from the conflicting opinions of witnesses, were obviously arrived at from other than scientific considerations, the question as to what is and what is not a coal, must be held to be an open one in those sciences in whose province the matter lies; and it will probably long remain so, because it was not from the absence of data, but from differing interpretations of facts about which for the most part there was a general accordance, that there arose the well known want of unanimity among geologists, mineralogists, chemists and microscopists.

In this paper I do not hope to decide the question, but I wish to point out as interesting facts the occurrence of true bituminous coal in contact with the Oil-Coal, and to call attention to the relative proportions of the ultimate elements in the latter and in the before-mentioned disputed substances as compared with bituminous coals, as important in explaining the different nature of their products of distillation, and in affording support to those who do not make one species only of these minerals.

Some of the analyses which follow are published for the first time; others, of my own, relating to bituminous coals, I have taken from among those given in the Report on Coals suited to the Steam Navy of Great Britain, by Sir H. Delabeche and Dr. Playfair, and those of cannel-coals are taken from sources hereafter indicated.

The oil-coal found near Pictou, N. S., was first met with by persons residing in the neighborhood, early in 1859, and its exact locality is called Fraser Mine. I occurs in the coal-measures. I am indebted to Henry Poole, Esq., manager of the Fraser Mine, for the following particulars relating to the geological position, etc., of the substance:—

"The lowest measures about sixty yards, on the surface, short of the distance where the oil-coal crops, are composed chiefly of strong bands of sandstone, actual thickness not yet proved; then shales with bands of ironstone, and Stigmaria roots with Sigillaria stems, and a few detached fern leaves, in such soft shale that I have not been able to preserve any good specimens. Immediately above the oil-coal is a seam of bituminous coal about fourteen inches thick. Where we commenced to open a mine by driving a slope, the oil-coal was fourteen inches thick, but at 200 feet down at the bottom of the slope the oil-coal was twenty inches thick; it has a smooth regular parting at top next the coal, as also at the bottom next to the Oil-Batt below, but throughout its entire thickness it is of a curly twisted structure; many of its fractures look like the casts of shells, and the sharp edges are polished of a "slickensides" character. No fossils that I am aware of have hitherto been found in the curly Oil-Coal. The Oil-Batt next below is nearly two feet thick, of a homogeneous character with a slaty cleavage of various thicknesses. In this band two or three varieties (species?) of Lepidodendron beautifully preserved have been found, also leaves about one-fourth of an inch wide, and in lengths of from four to six inches, which have undergone so little change, that when the damp shale was fresh split, they could be removed, and were so elastic that they could be bent considerably without breaking. At the bottom of the slope another thin seam of curly Oil-Coal has appeared of a few inches in thickness, but is not worked at present. In the roof-coal were found pieces of de-

cayed wood very little changed, which I consider a great curiosity. On M'Lellan's Brook shale is above the Oil-Coal, and Oil-Batt below in which have been found Lepidodendra and apparently molar teeth with three fangs, flattened modiola shells, and spines or small fish-teeth. The Oil-Batt has been found in several places without the curly band or so-called Oil-Coal. Two thousand tons of Oil-Coal have been raised (Dec. 1859) at the Fraser Mine."

The Oil-Coal varies in color from brown to black, is dull where not polished as just mentioned, has a reddish-brown lustreless streak, its powder is dark chocolate colored, it is very tough and breaks at last with a hackly fracture, its specific gravity, in mass, after the vessel of water containing it had been in an exhausted receiver =1·103. It takes fire very readily, and when removed from the lamp still burns for some time with a brilliant smoky flame, and flaming melted fragments continually drop from it in a truly characteristic manner. Ignited in coarse powder in an open crucible it gives off abundant smoke and flame, then seems to boil quickly, and a "coke" is left of the bulk of the original material, and showing when turned out a complete cast of the interior of the crucible. The ash of the "coke" is grey, and consists mainly of silicate of alumina, at least no lime or a mere trace is dissolved by acid, while some alumina is taken up and a great deal of solid remains undissolved. The powdered Oil-Coal digested with benzine and with ether does not more than sensibly color these flnids, but some residue remains on evaporation in each case.

The bituminous coal occurring with the oil-coal had the usual characters belonging to its species; it was black, brilliant, and very brittle. The proximate analyses of the two are placed side by side: and it will at once be obvious that they contrast very strikingly.

	Oil-coal.	Bitum. coal.
Volatile matters,	66·56	33·58
Fixed carbon,	25·23	62·09
Ash,	8·21	4·33
	100·00	100·00

The following is the ultimate analysis of the Oil-Coal, for which I am indebted to Mr. Slessor, assistant to Prof. Anderson of Glasgow, whose aid I requested from want of the necessary apparatus:

Carbon,	80·96
Hydrogen,	10·15
Nitrogen[a] (by loss),	0·68
Ash (as above),	8·21
	100·00

[a] with oxygen and sulphur?

The Oil-Batt appears to be decidedly a shale, and a specimen from Bear Brook, Frazer Mine, gave these results:

Volatile matters,	30·65
Fixed carbon,	10·98
Ash,	58·47=100·00

I proceed to compare the Torbane Hill mineral, and the "Albert coal." A specimen of the former, examined at the time of the trial before mentioned, in 1853, gave me—

Volatile matters,	71·17	Carbon,	66·00
Fixed carbon,	7·65	Hydrogen,	8·58
Ash,	21·18=100·00	Nitrogen,	0·55
		Sulphur,	0·70
		Oxygen,	2·99
		Ash,	21·18=100·00

In a recent examination, a specimen of "Albert coal" gave:

Volatile matters,	54·39	Carbon,*	87·25
Fixed carbon,	45·44	Hydrogen,	9·62
Ash,	0·17=100·00	Nitrogen,	1·75
		Ash,	0·17
		Oxygen and S,	1·21=100·00

These results I place in a table with corresponding data obtained from bituminous and cannel coals, the specific gravities of the substances, and the ratio of carbon to hydrogen as calculated directly from analysis, with the authority for the numbers. The first 7 analyses are from the Report on Coals by Sir H. Delabeche and Dr. Playfair, 1848, and Memoirs Geol. Survey, vol. ii; the 8th and 9th from Miller's Chemistry, iii, p. 201, and the 10th from Report of Trial on Torbane Hill "Coal," Edinburgh, 1853; the 11th has not been heretofore published.

	Name or Locality.	Sp. gr.	Proximate analysis. Volatile matters.	Fixed carbon.	Ash.	Ultimate analysis. C.	H.	N.	S.	O.	Ratio of C : H.	Observers.
†	Powell's Duffryn.	1·326	15·70	81·04	3·26	88·26	4·66	1·45	1·77	0 60	100 : 4 82	H. How.
†	Mynydd Newydd.	1·310	25·20	71·56	3·24	84·72	5·76	1·56	1·21	3·52	100 : 6·79	"
†	Ebbw Vale.	1·275	22·50	76·00	1·50	89·78	5·15	2·16	1·02	0·39	100 : 5·73	"
‡	Grangemouth.	1·290	43·40	53·03	3·52	79·85	5·28	1·35	1·42	8·58	100 : 6·61	"
‡	Fordel Splint.	1·25	47·97	48·03	4·00	79·58	5·50	1·13	1·46	8·33	100 : 6 93	"
§	Broomhill.	1·25	40·80	56·13	3·07	81·70	6·17	1·84	2·85	4 37	100 : 7·55	"
§	Parkend Sydney.	1·283	42·20	47·80	10·00	73·52	5·69	2·04	2·27	6·48	100 : 7·73	"
‖	Wigan.	1·276	39·64	57·66	2·70	80·07	5·53	2·12	1·50	8 08	100 : 6·90	Vaux.
¶	Lesmahagow.	1·251	56·70	37·26	6·034	73·44	7·62	?	1·145	?**	100 : 10 43	W. A. Miller.
¶	Capledrae.	?	?	?	25·40	56·70	6·80	1·90	0·35	8·80	100 : 11·99	A. Fyfe.
	Torbane Hill, Scot.	1·170	71·17	7·65	21·18	66·00	8·58	0·55	0·70	2·99	100 : 13·00	H. How.
	Hillsborough, N. B.	1·091	54·39	45·44	0·17	87·25	9·62	1·75	?	?‖†	100 : 11·02	J. Slessor and H. How.
	Pictou, N. S.	1·1039	66·53	25·23	8·21	80·96	10·15	0·68	?	‡‡	100 : 12·53	J. Slessor and H. How.

In this table we observe in the first place the resemblance of the last three substances in having a density much below that of

* This analysis kindly furnished by Mr. Slessor, as regards C, H, and N.
† Welsh bituminous coals. ‡ Scotch bituminous coals.
§ English bituminous coals. ‖ English cannel. ¶ Scotch cannel coals.
** N and O =11·761 per cent. ‖† S? and O =1·21. ‡‡ N, S, and O =0·68.

all the others, and secondly, that in all the bituminous coals but one the volatile matters are considerably less in amount than the fixed carbon, while in the cannel coals this is also the case with one of the two whose proximate analyses are given; as regards the other we see that it contains a large percentage (=11·761) of N and O which would of course be included as "volatile matters," and in the last three substances the volatile matters greatly exceed the fixed carbon. It is well known that in discussions on the chemical nature of coals, etc., much stress is laid on the relative proportions of these products, and also on the ratio of carbon to hydrogen, but it appears to me that an important element in the calculation has generally been omitted, or has not received due attention; I allude to the quantity of oxygen present, which of course can only be found by ultimate analysis. It is constantly stated that the gas and oil-producing value of a coal is indicated by the weight lost in coking, but this is obviously true only to a certain extent, and indeed is in some cases clearly untrue, for if we do not take into consideration the effect of oxygen present we cannot make a just comparison of the chemical nature of the substances, nor find the ratio of C : H, neither can we give the real gas or oil value, when, as above, from eight to ten per cent of what is generally supposed to be carbon and hydrogen is really oxygen with nitrogen. If for example we consider the effect of the oxygen in the composition of the substances given in the table we shall see the last three present such differences from the others as to strengthen the position of those who decline calling them 'coals.' Limiting our view to the cannel-coals, which, as seen above, exhibit the ratio of C : H apparently equal or nearly so to that in the substances in question, we observe that they all contain much more oxygen, and if we deduct the equivalent quantity of H in all, as is theoretically necessary for arriving at the heating power, we shall find this similarity greatly lessened; as thus,

			Ratio of C : H after deducting H=O.
Cannel coal	from	Wigan,	100 : 5·65
"	"	Lesmahagow,*	100 : 8·71
"	"	Capledrae,	100 : 10·05
Mineral	"	Torbane Hill,	100 : 12·43
"	"	Hillsborough,	100 : 10·85
"	"	Fraser Mine,	100 : 12·43

The last three substances should prove, theoretically, the excellent 'Oil-Coals' they are known to be. Of course the practical yield of oil will vary according to the manipulation, the perfection of the manufacturing processes and the quality of samples employed, but the following statement of the comparative

* After allowing two per cent for nitrogen.

amounts of oil afforded by some of the above may be taken as a good illustration of the point brought forward in this paper. I am indebted for these details to H. Poole, Esq.:—

In Scotland the Lesmahagow cannel coal gives 40 gallons crude oil, and 32 gallons rectified oil per ton.

At M'Lellan Brook the Fraser Oil-Coal gives 40 gallons crude oil per ton.

At Coal Brook the Fraser Oil-Coal and Oil-Batt together give 53 gallons per ton.

At M'Cullock Brook the Fraser Oil-Coal gives 77 gallons per ton.

The "Albert coal" gives 100 gallons per ton.

The "Torbane Hill coal" gives 125 gallons per ton.

And some picked samples of Oil-Coal from Fraser Mine, tried in Boston, U. S., gave no less than 199 gallons of oil per ton.

Windsor, Nova Scotia, May, 1860.

ART. XI.—*The Great Auroral Exhibition of Aug. 28th to Sept. 4th, 1859; and the Geographical Distribution of Auroras and Thunder storms.*—5TH ARTICLE. By Prof. ELIAS LOOMIS.

SINCE the publication of our former articles on the great aurora of Aug. 28th to Sept. 4th, we have received some additional observations which we here subjoin.

1. *Extract from a Journal of the weather in Swedish Bothnia,* (lat. 67° N., long. 22° E.), *by* ROBERT RAWLINSON, *copied from the London Times of Oct.* 5.

Aug. 27–28. Morning gloomy; clouds gray and electric looking; a sort of dense "Noah's Ark" sky.

Aug. 28. Night, heavy rain.

Aug. 29. Night bright and clear, but bitterly cold; ice a quarter of an inch thick round tent.

Aug. 30. Day fine; sun very hot. No observation at night.

Aug. 31. Day fine, clear and calm. No observation at night.

Sept. 1. Morning, heavy clouds showing for wet.

Sept. 2. Heavy thunder storm at night, vivid lightning and deluges of rain.

Sept. 3. Morning cloudy; thick mist over forest.

Sept. 4. Morning cloudy; heavy dew in night, thick fog.

Sept. 5. Cloudy, but fine.

2. *Observations at St. Petersburg, Russia*, (lat. 59° 56′), *communicated by* Prof. A. T. KUPFFER, *Director of the Central Physical Observatory.*

From Aug. 28th to Sept. 4th, 1859, the disturbances of the magnetic instruments at St. Petersburg were very remarkable not only for their extent but for their long continuance. The magnetic observations are ordinarily made every hour, but on the preceding days, observations were made every five minutes. We have not room to publish these observations entire, but the following table exhibits the most remarkable deflections of the magnetometers.

The north pole of the unifilar magnetometer moves towards the east, when the numbers of the scale increase. The value of one division of the scale is 26″·3.

When the numbers on the scale of the bifilar magnetometer increase, the magnetic intensity increases. The value of one division of the scale is 0·0001 of the total intensity.

In these observations the day is supposed to begin at noon.

Hour.	Unifilar.	Bifilar.
August 28th.		
h m		
0 0	132·5	185 4
3 0	105·8	193·7
9 0	143·0	207·0
13 0	108·0	186·0
16 0	169·3	420·0
19 0	122·0	420·0
22 0	192·0	420·0
23 05	105·0	106·7
23 20	165·0	230·2
23 40	102·0	127·5
August 29th.		
0 0	135·5	209·0
0 45	103·0	168·0
0 55	194·0	321·2
1 5	111·0	66·5
2 40	143·0	222·8
4 0	132·3	187·0
5 5	220·5	257·5
5 30	140·0	195·0
6 10	185·0	190·0
7 20	100·0	98·0
7 35	90·0	100·0
8 0	150 0	138·0
8 45	129·0	133·3
19 0	153·0	124·0
23 0	137·4	125·0
September 1st.		
0 0	101·2	136·5
2 0	75·5	183·0
9 0	150·8	167·9
12 0	120·0	159·0
20 0	151·5	419·0
20 30	410·5	419·0
21 0	530·0	419·0
21 10	601·5	419·0
21 30	302·0	216·0
21 40	9·0	12·0
21 45	off scale	7·8
21 55	94·0	0·0
22 5	off scale	119·0
22 15	320·0	177·0
22 30	85·0	0 0
22 50	123·7	98·5
23 15	10·0	150·0
23 30	128·0	100·0
23 45	59·5	86·0
September 2nd.		
0 5	135·0	205·3
0 35	67·0	150·0
0 45	134·5	65·0
1 55	81·0	178·0
2 20	186·0	393·0
2 25	82·0	555·0
2 40	off scale	304·5
3 20	134·0	306·0
3 40	13·5	519·2
4 0	147·0	292·5
4 45	85·0	420·5
5 0	122·4	571·5
5 10	41·0	501·0
5 50	104·5	277·5
6 45	100·0	172·2
8 0	234·5	420·0
8 5	242·0	303·4
8 25	115·0	166·0
10 35	95·3	90·3
12 0	70·1	50·2
12 15	63·0	10·2
12 25	47·3	off scale
12 50	160·9	70·6
13 35	156·0	off scale
14 45	116·2	78·0
15 15	122·4	44·6
16 20	113 0	108·0
17 5	98·0	104 2
18 35	160·0	109 5
20 20	147·0	129·0
20 55	127·0	123·0
21 45	150·3	115·2
22 5	136·0	106·3
23 55	120·0	136·0
September 3rd.		
0 15	126·0	143·0
0 50	111·9	127·5
1 55	85·0	247·0
2 55	75·5	210·0
4 15	50·0	237·0
4 55	45·0	229·0
6 45	110·0	275·0
8 0	144·0	142·0
8 40	44·0	55·0
9 0	101·0	52·5
10 0	158·0	154·0
13 40	72·0	off scale
14 20	65·0	35·5
16 20	157·0	135·2
18 15	150·0	140·0

Hour.	Unifilar.	Bifilar.	Hour.	Unifilar.	Bifilar.	Hour.	Unifilar.	Bifilar.
h m						*h m*		
20 55	127·0	84·0	September 4th.			5 10	142·5	158·0
21 20	84·2	109·0	*h m*			5 40	180·0	247·0
21 45	131·0	109·0	0 40	104·0	122·0	7 20	124·5	127·0
23 55	110·7	140·4	1 15	98·0	135·6	9 35	113.5	150·0
			2 35	113·8	134·0	16 0	140·2	134·2
			3 20	79·0	233·0	20 0	126·3	159·0
			4 0	101·5	257·0	23 0	115·0	136·0

From the preceding observations we see that the range of the magnetometers for each day was as follows:

	Unifilar magnetometer.	Bifilar magnetometer.
	d d d	*d d d*
Aug. 28	From 102 to 192 = 90 = 39′ 27″	From 106·7 to 420 = 313·3 = ·031
Aug. 29	" 90 " 220·5 = 130·5 = 57 12	" 66 5 " 321·2 = 254·7 = ·025
Sept. 1	" 0 " 601·5 = 601·5 = 263 39	" 0 " 419·0 = 419·0 = ·042
Sept. 2	" 0 " 242 = 242 = 107 44	" 0 " 571·5 = 571·5 = ·057
Sept. 3	" 44 " 158 = 114 = 49 58	" 0 " 275 = 275 = ·027
Sept. 4	" 79 " 180 = 101 = 44 16	" 122 " 257 = 135 = ·013

It should be remarked that in the preceding table, 0 indicates that the magnets passed beyond the range of their scales; so that we can only conclude that on Sept. 1st and 2nd the range of the magnets certainly *exceeded* the values here given.

3. *Observations at Catherinenburg, Russia* (lat. 56° 50′, long. 60° 34′ E.), *communicated by* Prof. A. T. KUPFFER.

The following observations are arranged in the same manner as those from St. Petersburg. The value of one division of the unifilar scale is 33″·4. The value of one division of the bifilar scale is 0·0001 of the total intensity.

Hour.	Unifilar.	Bifilar.	Hour.	Unifilar.	Bifilar.	Hour.	Unifilar.	Bifilar.
			h			*h*		
September 1st.			22	off scale	off scale	8	536·5	300·5
h			23	off scale	180·0	10	481·0	295·7
0	496·0	308·5	September 2nd.			11	519·0	297·0
2	490·0	311·6				14	512·0	283·0
6	507·5	319·0	0	434·0	238·0	20	539·0	285·4
12	510·3	321·0	3	519·0	281·2	23	528·0	291·8
18	525·0	320·2	6	427·0	396·0			
21	580·0	201·0	7	520·0	412·0			

4. *Observations at Barnaul, Russia* (lat. 53° 20′, long. 83° 27′ E.), *communicated by* Prof. A. T. KUPFFER.

The value of one division of the unifilar scale is 32″·8. One division of the bifilar scale =0·0001 of the total intensity.

Hour.	Unifilar.	Bifilar.	Hour.	Unifilar.	Bifilar.	Hour.	Unifilar.	Bifilar.
	August 28th.			August 29th.			September 1st.	
h			h			h		
0	391·5	171·6	0	416·5	off scale	22	413·2	152·5
1	383·6	175·4	2	402·5	20 0	23	519·3	off scale
4	392·6	185·0	3	382·0	121·0		September 2nd.	
10	402·3	171·4	5	420·0	48·0	0	off scale	466·5
14	408·0	161·5	6	437·0	94·0	2	358·5	32·3
18	423·4	69·0	9	404·0	116·0	6	385·0	262·0
20	431·8	7·2	10	422·0	106·5	8	350·6	156·5
23	412·5	off scale	18	417·5	125·0	9	440·5	77·4
			23	408·5	107·5	19	397·0	166·6
						23	404·0	107·5

5. *Observations at Nertchinsk, Russia,* (lat. 51° 18′, long. 119° 20′ E.), *communicated by* Prof. A. T. KUPFFER.

The value of one division of the unifilar scale is 33″·8. One division of the bifilar scale =0·0001 of the total intensity.

Hour.	Unifilar.	Bifilar.	Hour.	Unifilar.	Bifilar.	Hour.	Unifilar.	Bifilar.
	September 2nd.		h			h		
			5	294·8	205·6	11	450·0	260·0
h			6	327·1	224·1	12	333·0	246·0
0	347·0	285·1	7	339·5	256·5	13	345·4	243·0
1	383·0	235·4	8	335·0	250·4	17	327 0	269·0
2	635·4	370·1	9	312·2	288·5	19	352·0	279·0
3	355·5	321·5	10	311 9	243·5	23	343·5	243·0
4	214·0	181·0						

6. *Observations at Athens, Greece* (lat. 37° 58′), *by* J. F. L. SCHMIDT, *Director of the Observatory.* (Communicated through Rev. Mr. King, Am. Missionary at Athens.)

The aurora was not seen at Athens Aug. 28th and 29th. Both evenings were very clear and still, especially Aug. 28th. Aug. 29th from 7h to 8h P. M. some clouds were seen in the west over the Morea. Aug. 30–31 was likewise clear, with a very few small clouds. Aug. 31st, in the evening, lightning in the N.W. Sept. 1st, evening, partly clear, partly cloudy, with lightning in the west.

Sept. 2d, 7h 15m A. M., beginning of a storm from the west, rain and thunder; at 8h 30m A. M., rain, hail and lightning. From noon to 12h 40m P. M., violent shower from the west. Then became clear with sunshine. The evening was clear, and in the north there appeared a dark bank of ordinary cloud (not the dark segment of the aurora), above which, from 7h 30m P. M. to 8 P. M., was seen a fine aurora of a carmine red color. The cloud bank, which extended 60° in azimuth, was elevated somewhat above the horizon, so that stars were seen beneath it. The centre of the auroral light was not in the north, but N.N.W. On the west it was bounded by Cor Caroli, and on the east by

α Persei. No streamers or fluctuations of light were observed. By 10 P. M. the cloud bank had disappeared, the auroral light having disappeared previously.

Sept. 3d. The entire day was clear; and at 4 P. M. I went on board a steamer for Syra. From 7½–8 P. M., near the island Egina I saw in the north and northwest the carmine red light of an aurora. From 9½–10½ P. M., near Cape Sunium, a faint trace of the aurora was still seen; but no dark segment, streamers or fluctuations of light.

7. *Observations at Camp Simeahmoo, Washington Territory,* (lat. 49°, long. 122° 30′ W.), *by* ARCHIBALD CAMPBELL, *Commissioner of N. W. Boundary Survey.*

At 8 P. M. Aug. 28, 1859, a diffused light, without definite form, was observed a little east of north, covering about one-fourth of the heavens, which gradually increased to the west, sending across from east to west an arch of a whitish color, the arch itself being much brighter than the circumjacent light. This arch remained visible until 2 A. M. At 9^h 25^m P. M. strongly marked rays became visible, which rising from the horizon converged to a point on the arch a little south of the zenith, and in this position remained visible about one hour. The rays in the northwest were of a pink color, those in the southeast were purple, alternately brightening and fading to a whitish color. At midnight, all disappeared except the arch, and at intervals undulating flashes of light appeared, not visible longer than three seconds. Occasionally streamers shot up from the horizon, the lower part disappearing before the upper part had reached the zenith. Sometimes these streamers were broad at the horizon, and came to a point near the zenith, and sometimes the reverse. The arch before mentioned was easily identified, and was still visible at 2^h A. M., and probably remained so until daylight, which at that season, in this latitude, occurs not long after that time. This arch was situated very little, if any, to the southward of the zenith, and was the limit of all light in that direction. The light was sufficiently intense, between 11 and 12 o'clock, to enable a person to read the ordinary print of a newspaper. After the aurora was fully formed, it remained stationary, and did not move either to the west or east. At midnight, the barometer stood at 30·13; external thermometer 64° F.

August 29th a faint diffused light was seen in the north at 9 P. M., and was still visible at midnight.

August 30th a similar light was first seen at 8½ P. M. and was still visible at midnight. No observations were made after midnight. There was no exhibition of the auroral light at this place from Aug. 31st to Sept. 4th.

The view of the northern horizon at this place is cut off by a dense forest of firs, and the sight of the heavens in that direction is some 5 or 6 degrees above the horizon.

8. *Observations at Hamilton, C. W.* (lat. 43° 16′ N.), *by* Dr. J. HURLBURT.

Answers to the questions in our first article, vol. xxviii, p. 408.

1. On the 28th of August the sky was overcast by a dark sombre cloud-like substance, but which was not cloud as the stars could be seen through it. There was no dark segment resting on the northern horizon, but one was seen at the south between 8 and 9 P. M. rising 8° or 10° in the centre. Sept. 1st between 8 and 9 P. M. this dark segment was well defined at the north, with an altitude of about 10°, and skirted the horizon fully 120°. At 1 A. M. Sept. 2d, an unusually large segment was distinctly defined at the south, where it rose fully 15° in the centre, and stretched over 130° of the horizon.

2. At 8 P. M. Aug. 28th, and from 1 to 3 A. M. Sept. 2d, the streamers of the aurora converged to a point a little east of the meridian, and 15° or 20° south of the zenith, forming a brilliant corona.

3. Aug. 28th, both in the southwest and southeast there was a dark red spot about 14° in breadth, and extending from altitude 35° to alt. 55°. Both spots presented the same appearance, and hung in corresponding parts of the heavens east and west.

4. At 1 A. M. Sept. 2d, the whole of the southern half of the sky was lighted up, resembling the sky at late dawn.

5. On the night of Aug. 28th, the most frequent and conspicuous color was red and its different shades. The aurora of Sept. 1st was scarcely at all marked with any of these colors. The light was chiefly white, resembling the dawn just before sunrise.

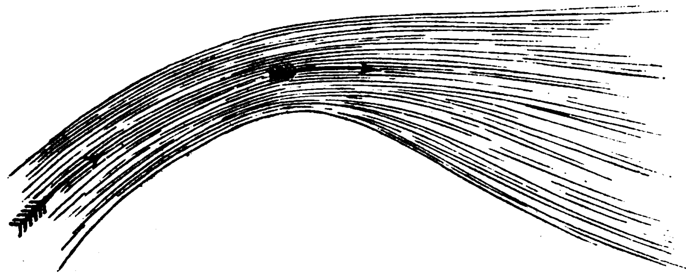
6. The aurora of Aug. 28th was most brilliant at 8 P. M. It was also very brilliant at 1 and 3 A. M. Aug. 29th. The aurora of Sept. 2d was most brilliant at 1 A. M.

9. *Observations at Rome, N. Y.* (lat. 43° 13′), *by* Mr. EDWARD HUNTINGTON.

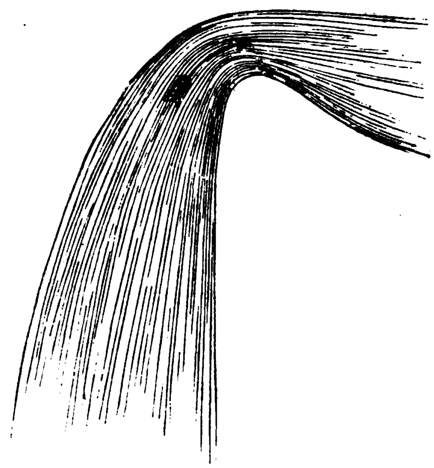
During the auroral display on the evening of Sept. 2d, there was a very rapid and incessant flashing of white light, like waves running up to the corona, the light being intermitted for some seconds after the passage of each wave, and the next wave pursuing the same course, and *following certain curvatures which remained constant for a considerable time*, perhaps half an hour. There was a succession of waves flashing up from a point in the northwest, where the effect was as if a luminous fluid were in its course passing through a narrow strait, from which it issued,

expanding like water violently forced through a narrow passage out upon a broad level area, and being at the same time somewhat deflected from its former course. Fig. 1 is designed to convey some idea of this appearance.

1.



2.



From another point nearly in the east, there was a succession of waves of light, directed to a point north of the corona, but turning suddenly into it nearly at a right angle with their former course, the light disappearing after the passage of each wave, and being renewed after a short interval in precisely the same form. Fig. 2 is designed to give some idea of this appearance.

10. *Observations at Cleveland, Ohio* (lat. 41° 31′), *by* Capt. B. A. STANARD.

Aug. 28th, at 8^h 30^m P. M., the aurora began to show itself, lighting up the northern sky, rising towards the zenith, in a broad belt of luminous haze of about 40° in width, the southern edge starting a little to the westward of the star Arcturus, and running through the head of Hercules, a little south of Altair in Aquila, through the head and neck of Pegasus. The eastern end was bright enough to light up the edges of the detached masses of cumuli that were driving over from the north. In the zenith and the western end it was of moderate brightness.

At 9 P. M. another belt began to rise up in the north, and as the convex edge attained a height of about 40° it began to shoot out long, attenuated, bright rays, close together, moving slowly to the westward, and reaching to the zenith. Near the convex edge they were of a bright yellow, changing as they shot up to

orange, and near the zenith to a bright red, the middle and lower ends remaining yellow and orange. As the fiery points of the rays shot into the broad belt overhead, which had still remained like a belt of luminous haze or fog, the whole thing was changed in an instant into a bright red color, deepening as it neared the eastern horizon, to a bright crimson, and at the western end, near the star Arcturus, into a bright scarlet, gradually growing fainter in the zenith, and increasing in brightness nearer the horizon.

At 9^h 15^m P. M. it resolved itself into converging rays. The zenith at that time was covered by a massive cloud, covering the point where the rays would meet, which must have been near the star β Cygni.

At 9^h 30^m P. M. the whole had disappeared, except a steady white light in the north, very bright, forming a curve from the western edge of Böotes, through Cepheus and Perseus. The northern horizon was too clouded the whole time to see anything near it.

Sept. 2nd, at 7^h 45^m P. M., the northern lights began to show, forming a low narrow arch extending from the N. W. to N. E., the lower edge about 10° in height, well defined, of moderate brightness, and remaining without perceptible change until 8^h 45^m P. M., when bright vertical rays shot up simultaneously the whole length of the arch, of an exceedingly bright white color, with no perceptible motion E. and W. Shooting slowly up towards the zenith, at an altitude of about 45°, they began to change color rapidly, varying from white, yellow, orange, and at the upper ends red; diffusing a soft pink color over the northern sky at an altitude of about 45°. At 9 P. M. they gradually disappeared, the arch was broken up, leaving some irregular white blotches in the north, which faded away and disappeared altogether. Then commenced a series of quick, sudden flashes of undefined light; here and there in the north, scimetars bearing a strong resemblance to heat lightning, sometimes in undefined rays, and sometimes in undefined shimmering light.

This continued until 9^h 45^m P. M., when a double arch was formed of two narrow belts of light about 15° in height, running from Canes Venatici to the southern edge of Perseus, the bright star Capella shining through the narrow black space between the two arches.

At 9^h 55^m P. M. bright rays suddenly shot up in quick successive flashes from the lower through the upper arch, reaching nearly to the zenith, and moving slowly to west until they reached the constellation Corona Borealis, lighting up the north western sky with yellow, orange and red. After the last rays from the east had passed the Pole, there commenced a sudden flashing of horizontal wavy bands from the upper arch towards the zenith.

At 10^h 10^m P. M. the rays and arches disappeared and the northern sky seemed to be covered with a steady white light, with horizontal wavy bands of dark haze rolling up in quick succession, and vanishing as they attained an altitude of about 60°, continuing until 10^h 30^m P. M. and gradually fading away.

11. *Observations at Fort Bridger, Utah Ter.* (lat. 41° 14′, long. 110° 33′), *by* KIRTLEY RYLAND, *Assist. Surgeon U. S. Army.*

Sept. 1st, 1859, a brilliant aurora was seen at this place. It was first observed about 11 P. M. and attained its greatest brilliancy before midnight. It extended from the northeastern horizon to the southern horizon, and was in fact a Borealo-Austral Aurora. Generally the light assumed the form of spikes and bars, but high above the horizon in the northeastern sky was a large blotch or spot, whose diameter was perhaps three times that of the constellation Orion. This blotch was of a deep crimson color, and remained for a considerable time unchanged in form, color or intensity, and faded gradually away. In the other portions of the aurora the light appeared to spout from the horizon, in the shapes already named, frequently reaching the zenith, and was of great brilliancy. It appeared to flow gradually from N.N.E. to the southern sky.

12. *Observations made at Cantonment Burgwin, New Mexico* (lat. 36° 21′, long. 105° 42′), *by* W. W. ANDERSON, *Assist. Surgeon U. S. Army.*

A member of the guard mounted Sept. 1st observed a light reflected from the clouds on that night about 10 P. M. as he was walking post, the clouds having probably just then thinned out or broken away a little. No other member of the guards from Aug. 28th to Sept. 5th saw anything unusual during the intervening nights. By referencc to the Meteorological Register I find that the weather was cloudy during the whole time that the aurora was visible elsewhere. There was rain on the last four days of August, and also on the 1st, 2nd and 4th of September.

The exhibition was witnessed at Taos, about ten miles north of us, by persons residing there, but was not observed with sufficient attention to enable them to answer any of your questions with accuracy. A physician, Dr. Ferris, who arrived at Taos from Pike's Peak during the fall, states that he was in the South Park at the time, and thinks that it was on the night of the 28th of August that the Aurora was observed by him. He saw it but one night. Streamers were seen to converge to the zenith about 10 P. M., but the aurora was not seen in the southern half of the heavens. The light was like a large fire in the distance, so that at first it was thought to be an extensive fire on the

mountains. The aurora exhibited sudden flashes, and there were pulsations like waves of light rushing up from the horizon. I have been told that Capt. John G. Walker, of the Rifle Regiment, wrote a description of the aurora as it appeared at Fort Defiance (lat. 35° 44′), where it is said to have been very brililant.

13. *Observations at the Sandwich Islands* (lat. 20° N., long. 157° W.), *from the Pacific Commercial Advertiser.*

The Advertiser of Sept. 8th, 1859, states: "There was quite a display of the Aurora a few nights since, visible at Honolulu. Broad fiery streaks shot up into and played among the heavens, almost as beautifully as those which are sometimes seen in more northern climes."

The Advertiser of Sept. 17th contains the following letter from S. E. Bishop, dated Lahaina, Sept. 9th. "Your statement that the Aurora was seen in Honolulu enabled me to account for the phenomenon I observed here a few nights since. At 10 P. M. I noticed a bright, unsteady crimson glow upon the sky, extending from N. E. to N., and about 35° of altitude. It resembled the reflection of a great conflagration at twenty or thirty miles distance, and I attributed it to heavy fires on the other side of the mountain. I was puzzled however by the fact that the clouds which rested on the mountain did not give the slightest reflection of the supposed fire. Moreover the light was far too pure and rich a crimson to have been caused by a fire.

14. *Observations at Porto Rico, West Indies* (lat. 18° N.), *by* M. du COLOMBIER, *from L'Institut of Feb. 1st,* 1860.

Having awakened at $2^h\ 30^m$ A. M., Sept. 2nd, I was greatly astonished to see my windows, which looked towards the north, brightly illumined by a brilliant purple light. Rising immediately, I perceived that this light proceeded from a magnificent aurora, which, according to the testimony of the guard, commenced at 2^h A. M. and was observed till 4^h A. M. The luminous rays, red, purple and violet, extended even to the zenith. The oldest inhabitants of the place declared that they had never before seen a phenomenon of this kind.

15. *Observations at Santiago de Chili* (lat. 33° 26′ S.), *by* C. MOESTA, *Director of the Observatory.*

The aurora you allude to did occur at this and several other places in the south of Chili, during the night between Sept. 1st and Sept. 2nd, 1859. I did not witness the phenomenon myself, but it appears that the aurora was visible from about half past 1 until 4 A. M., showing a motion to the west. The watchmen were much alarmed at the colored light with which the southern

part of the sky was covered, which gave rise to the belief that a small village about three leagues south of Santiago was on fire. This seems to be the first time that a polar light has been seen at Santiago. No notice has reached me as to its appearance north of this place.

16. *Observations near Cape Horn* (lat. 57° S., long. 66° W.), *by* RICHARD SCHUMACHER, *communicated by C. Moesta.*

Mr. Richard Schumacher, assistant to the Chili Observatory, was at the time on board a ship near Cape Horn. Being informed Aug. 29th that an aurora had been seen during the preceding night, he begged the mate of the vessel would let him know if there should be another. Accordingly he was awakened during the night from Sept. 1st to 2nd, between 2 and 4 A. M., when the aurora was already in its splendor. In a southerly direction there appeared a bright yellowish light forming an ellipse, whose diameters were as two to one, the centre of the ellipse being elevated about 15° above the horizon. The part of the horizon below this light seemed to be a cloudy mass of a dirty reddish color. From this ellipse, emanated a red light, apparently all over the heavens up to the zenith, and thence onwards to the north. Mr. S. did not distinguish any beams or columns of light, though the sky seemed illuminated all round nearly uniformly, except that the light of the ellipse was much brighter than the surrounding parts. There were also some light transparent clouds discernible near the zenith. The light was so brilliant that he could easily read the title page of the Nautical Almanac, and distinguish the seconds hand of a box chronometer.

The vessel was at noon, Sept. 1st, in lat. 57° 8′, long. 66° 38′ W.
" Sept. 2nd, " 57 36 " 66 47 "

ON THE GEOGRAPHICAL DISTRIBUTION OF AURORAS IN THE NORTHERN HEMISPHERE.

Auroras are very unequally distributed over the earth's surface. They occur most frequently in the higher latitudes, and are almost unknown within the tropics. At Havana (lat. 23° 9′) but six auroras have been recorded within a hundred years; and south of Havana, auroras are still more unfrequent. As we travel northward from Cuba, auroras increase in frequency and brilliancy; they rise higher in the heavens, and oftener attain the zenith. The following tables furnish the most precise data I have been able to collect for constructing an auroral chart of the northern hemisphere. Column first gives the name of the station of observation; columns second and third its latitude and longitude; column fourth the average number of au-

roras observed per year; column fifth, the greatest number of auroras recorded in a single month; column sixth shows the number of years embraced in the comparison; and column seventh shows the authority for the statement.

The numbers for several of the stations are derived from an article contained in vol. viii of the Smithsonian Contributions, entitled, Record of auroral phenomena observed in the higher northern latitudes, compiled by Peter Force. Such observations are indicated by the word 'Force' in the last column.

TABLE I.—*Average annual number of Auroras in North America and its vicinity, from longitude* 30° *to* 170° *west from Greenwich.*

Place.	Latitude. ° ′	Longitude. ° ′	Per year	Richest month.	Years embraced.	Authority.
Havana,	23 9	82 22	1/16	1	100	Am. Jour., [2], xxviii, 408.
San Francisco,	37 47	122 26	½	1	4	Smithson. Report, 1854, p. 258.
Sacramento, Cal.,	38 34	121 27	½	1	9	Am. Jour., [2], xxix, 260.
Washington, D. C.,	38 53	77 0	9	3	4	Gilliss's Met. Observations.
Wilmington, Del.,	39 44	75 32	8	4	7	Am. Jour., xxxiii, 299.
Philadelphia,	39 58	75 10	6	3	4	Bache's Met. Observations.
New York,	40 42	74 0	14	5	1	N. Y. Regents Rep., 1850, 255.
New Haven,	41 18	72 55	26	12	16	E. C. Herrick's observations.
North Salem, N. Y.,	41 26	73 38	8	2	10	N. Y. Regents Rep., 1850, 258.
Deerfield, Mass.,	42 33	72 35	4	2	1	Am. Jour., iv, 337.
Fayetteville, Vt.,	42 58	72 40	21	8	11	Am. Jour., vols. xii to xxiv.
London, C. W.,	42 58	81 25	27	7	4	{ Regents Rep., 1850, 290–293. { Am. Jour., [2], xiv, 156.
Toronto, C. W.,	43 39	79 23	39	9	12	" " "
Kingston, C. W.,	44 8	76 40	86	11	4	Am. Jour., [2], xiv, 156.
Somerville, N. Y.,	44 26	75 20	70	14	2	" " "
Halifax, N. S.,	44 39	63 36	55	13	2	" " "
Montreal, C. E.,	45 31	73 32	34	10	2	Regents Rep., 1850, 290 & 291.
Quebec, C. E.,	46 49	71 12	42	12	4	" " "
St. John's, N. F'nd,	47 33	52 38	52	10	3	" " "
Michipicoton,	47 56	85 2	43	9	2	Am. Jour., [2], xiv, 156.
Matawagomingen,			26	7	1	" " "
Moose Factory,	51 10	81 0	141	19	1	" " "
Martin's Falls,	51 52	86 45	79	14	1	" " "
Cumberland House,	53 56	102 16	104	25	2	Gehler's Wörterbuch, vii, 144.
Athabasca Lake,	58 43	111 18	91	21	2	{ Athabasca Obs., p. 145. { Am. Jour., [2], xiv, 156.
Frances Lake,	61 30	129 0	33		2	Lake Athabasca, p. 148.
Lewis and Pelly,	61 30	130 0	36	12	1	Am. Jour., [2], xiv, 156.
Fort Simpson,	61 51	121 32	50	24	1½	AthabascaObs.&Am.J.,xiv,156.
Great Slave Lake,	62 46	109 1	105	28	2	Capt. Back, 1833–1835.
Godthaab,	64 10	51 53	72	15	6	Observat.Meteorolog., 166–228.
Fort Enterprise,	64 28	113 6	142	28	1	Force, pp. 24-35.
Fort Norman,	64 40	124 45	32	15	½	Force, p. 54.
Fort Franklin,	65 12	123 12	43	17	½	Force, pp. 60–64.
Youcon,	66 0	147 0	24	7	½	Am. Jour., [2], xiv, 156.
Winter Island,	66 11	83 10	25	8	1	Force, pp. 66–72.
Fort Hope,	66 32	86 56	39	13	1	Force, pp. 75–77.
Fort Confidence,	66 54	118 49	100	30	2	{ Athabasca Obs., 324–350. { Am. Jour., [2], xiv, 156.
Peel's River,	67 27	134 30	65	16	1	Am. Jour., [2], xiv, 156.
Jakobshaven,	69 12	51 0	18	5	8	Observat. Meteorolog., 82–164.

TABLE I.—*Continued.*

Place.	Latitude. ° ′	Longitude. ° ′	Per year	Richest month.	Years embraced.	Authority.
Felix Harbor,	69 59	92 1	23	10	1	Force, pp. 84–87.
Point Barrow,	71 21	156 15	131	26	1	Phil. Trans., 1857, p. 497.
Port Bowen,	73 14	88 55	47	15	1	Force, p. 91.
Melville Island,	74 47	110 49	26	8	1	Force, pp. 97–103.
Smith Sound,	78 37	70 40	10	5	2	Dr. Kane's Obs.

The following auroral observations are generally confined to brief periods of time, and have therefore less importance than the preceding. They are nevertheless of some value in deciding in what part of the world auroras are most frequent and brilliant. These notices are all taken from Force's article in the Smithsonian Contributions, vol. viii.

TABLE II.—*Notices of Auroras in North America and its vicinity, from longitude 80° to 170° west from Greenwich.*

Place.	Latitude. ° ′	Longitude. ° ′	Auroras.
Cedar Lake,	58 13	100 10	Extremely brilliant and covered the whole sky.
Off Cape Farewell,	56 17	42 51	Frequently most brilliant.
At sea,	57	49	Occurred almost every night.
York Fort,	57 2	93	Very few winter nights without the aurora. One may read distinctly by it.
At sea,	57 30	45	Whole southern hemisphere illumined. Gave nearly as much light as the full moon.
At sea,	58 12	49 15	Radii shot from the southward.
At sea,	58 30	44 30	Far surpassed anything of the kind observed at Port Bowen.
At sea,	59	50	Seen in every part of the heavens.
At sea,	59 58	59 53	Brilliant coruscations.
Hoarak,	59 59	44 36	Four luminous arches.
At sea,	60	56	Whole sky illuminated.
Kikkertak,	60 4	43 2	Unusually brilliant coruscations.
Nennortalik, Greenland,	60 8	45 16	Of every day occurrence. Brighter than the full moon.
Davis Straits,	60 10	49 40	Yellow and reddish coruscations, extending near the zenith.
At sea,	61 4	49 50	Illumined the whole southern sky.
Davis Straits,	61 37	52	The whole sky was one living fire of aurora.
Davis Straits,	61 58	54 40	Most brilliant aurora danced to the zenith.
At sea,	62 30	63	Aurora seen in the south.
Rankin's Inlet,	62 35	93	Aurora very bright, spreading all over the sky.
Hudson Strait,	62 45	72 24	Aurora unusually splendid.
Good Hope,	64 10	51 42	Auroras always spring up in the E. or S.E.
Cape Lavenorn,	64 30	39 30	Auroras unfrequent.
At sea,	65	63	Very brilliant, spread all over the heavens.
Southampton Island,	65 28	84 40	Visible during the whole of the night.
At sea,	65 50	61	Very brilliant; shooting rays to the zenith.
Hudson's Bay,	66 11	82 53	Very brilliant.
Chamisso Island,	66 13	161 47	Very brilliant.
At sea,	66 30	59	Seen in the south.
Kotzebue's Sound,	66 30	163	Aurora always seen to the northward.
Fort Macpherson,	67	135	Six auroras seen in fifteen days.

TABLE II.—*Continued.*

Place.	Latitude. ° ′	Longitude. ° ′	Auroras.
Cape Krusenstern,	67 8	163 46	Shot up to the zenith; pink, purple, and green rays.
Fort Good Hope,	67 28	130 54	Spread all over the sky.
Behring's Sea,	68 30	167	Unusually brilliant display.
Hearne's Sea,	68 48	115 31	Most superb display.
Igloolik,	69 15	81 45	2 auroras in Nov.; 1 in Dec. 1822. Generally faint.
Baffin's Bay,	70 43	63 44	Faint aurora to southward.
Baffin's Bay,	71 20	62 28	Brilliant aurora.
Baffin's Bay,	72 10	68 36	Eleven auroras seen in Feb. 1851.
Somerset House,	72 48	95 41	Seldom seen in 1833.
Baffin's Bay,	72 49	70 59	Nine auroras seen in Jan. 1851.
Batty Bay,	73 17	91	Great luminous rays issued from the zenith.
Austin's winter quarters,	74 10	94 16	11 auroras in Jan. 1851; 12 in Feb. 1851; and 4 in March, 1851.
Lancaster Sound,	74 18	82 10	Eleven auroras in Dec. 1850.
Griffith Island,	74 30	95 20	Vastly inferior to those seen in more southern latitudes.
At sea,	74 31	111 38	Aurora faint.
Off Beechy Island,	74 40	92	Ten auroras seen in Nov, 1850.
Assistance Harbor,	74 40	94 16	Much less vivid than in more southern latitudes.
Barrow Strait,	74 45	94	Two auroras in Sept. 1850.
Wellington Channel,	74 54	93	Seven auroras seen in Oct. 1850.
Northumberland Sound,	76 52	97	Five auroras seen in Dec. 1852.

The following table exhibits the average annual number of auroras in Europe. The table is arranged in the same manner as Table I.

TABLE III.—*Average annual number of Auroras in Europe.*

Place.	Latitude. ° ′	Longitude. ° ′	Per year	Richest month.	Years embraced.	Authority.
Bologna,	44 32	11 23 E.	4	6	24	Mairan Aurore Boreale, p. 505.
Paris,	48 50	2 20 E.	19		64	Gehler Wört., vii, 1, 135.
Montmorency,	49 0	2 19 E.	5	4	27	Cotte Meteorologie, 355.
Carlsruhe,	49 1	8 25 E.	8	5	11	Gehler Wört., vii, 1, 146.
Plymouth,	50 22	4 9 W.	6	5	21	Am. Journal, xxxiii, 298.
Leyden,	52 9	4 29 E.	26		29	Cotte Meteorologie, 355.
Berlin,	52 30	13 23 E.	4	5	28	Mairan Aurore Boreale, 500.
Franecker,	53 12	5 32 E.	25	8	7	Cotte Meteorologie, 355.
Kendal,	54 19	2 45 W.	31	10	7	Dalton's Met. Essays, 54–58.
Makerstoun,	55 35	2 31 W.	31		6	Am. Jour., [2], xi, 139.
Dunse,	55 47	2 20 W.	24	11	10	Phil. Trans. Abstracts, vi, 291.
Upsala,	59 52	17 38 E.	37	14	21	De la Rive Elec., iii, 301.
Christiana,	59 54	10 43 E.	33	13	16	De la Rive Elec., iii, 300.
St. Petersburgh,	59 56	30 18 E.	21	8	11	Mairan Aurore Boreale, 510.
Bossekop,	69 58	23 34 E.	143		1	Pouillet Physique, ii, 663.

The following observations are less definite than the preceding, and are therefore given in a separate table.

TABLE IV.—*Notices of Auroras in Europe and its vicinity from Longitude* 60° *East to* 30° *West from Greenwich.*

Place.	Latitude. ° ′	Longitude. ° ′	Auroras.	Authority.
Teneriffe,	28 16	16 39 W.	Aurora seen Nov. 1837 for the first time in the memory of man.	Alfred Diston.
Shetland Islands,	60 20	1 9 W.	Constant attendant of clear evenings.	Rees' Cyc., v. iii.
At sea,	60 30	25 W.	A bright arch with coruscations.	Force, p. 13.
At sea,	61	25 W.	Passed through the zenith and eclipsed the moon in splendor.	Force, p. 14.
Iceland,	64	22 W.	Seen almost every clear night.	Henderson, p. 148.
Torneo,	65 52	24 13 E.	Frequently occupy the whole sky. Commonly seen in the north.	Maupertuis, ii, 155.

The following are the most definite observations I have been able to collect from the Asiatic continent.

TABLE V.—*Auroras in Asia and its vicinity from Longitude* 60° *East to* 170° *West from Greenwich.*

Place.	Latitude. ° ′	Longitude. ° ′	Auroras.	Authority.
Irkutsk,	52 20	103 50 E.	Two auroras seen in Dec. 1735.	Gmelin, p. 434.
Tomsk,	56 35	86 30 E.	Only one aurora per month in 1741.	Gmelin, p. 477.
Catherinenberg,	56 50	63 35 E.	Four auroras seen in 1854.	Kupffer Obs.
Turinsk,	57 45	63 45 E.	Three auroras seen in March 1742.	Gmelin, p. 326.
Kirenskoi Ostrog,	58 0	108 E.	Five auroras seen in March 1739.	Gmelin, p. 458.
Tobolsk,	58 12	68 18 E.	Not much more frequent than under the same latitude in Europe.	Erman, i, 394.
Jeniseisk,	58 30	92 E.	Three auroras seen in Feb. 1738.	Gmelin, p. 453.
Beresov,	63 56	65 4 E.	Sometimes seen for months together throughout the night.	Erman, i, 470.
Virchni Koorina,	66	152 E.	Constant and very brilliant.	Billings, p. 57.
Koliutchin Island,	67 26	175 35 W.	More frequent and brilliant than at Nijnei Kolymsk.	Von Wrangell, 506.
Nijnei Kolymsk,	68 32	160 56 E.	Seen almost every evening.	Von Wrangell, 83.
On the ice,	69 58	168 41 E.	An aurora of extraordinary beauty.	Von Wrangell, 103.
On the ice,	70 20	174 13 E.	Beautiful aurora all night.	Von Wrangell, 318.

If we project all the preceding observations upon a chart, we shall discover considerable uniformity in the distribution of auroras over the earth's surface. If we travel from the equator northward along the meridian of Washington, we find on an

average near the parallel of 40°, only ten auroras annually. Near the parallel of 42°, the average number is 20 annually; near 45°, the number is 40; and near the parallel of 50° it amounts to 80 annually. Between this point and the parallel of 62°, auroras are seen almost every night. They appear high in the heavens, and as often to the south as the north. Further north they are seldom seen except in the south, and from this point they diminish in frequency and brilliancy as we advance towards the pole. Beyond lat. 62° the average number of auroras is reduced to 40 annually. Beyond lat. 67° it is further reduced to 20, and near lat. 78° to 10 annually. If we make a like comparison for the meridian of St. Petersburg, we shall find a similar result, except that the auroral region is situated further northward than it is in America; the region of 80 auroras annually being found between the parallels of 66° and 75°.

Upon the accompanying chart, the deep red color indicates the region where the average number of auroras annually amounts to at least 80; and the pale red color indicates the region where the average number of auroras annually amounts to at least 40. We thus see that the region of greatest auroral action is a zone of an oval form surrounding the north pole, and whose central line crosses the meridian of Washington in lat. 56°, and the meridian of St. Petersburg in lat. 71°. Accordingly auroras are more frequent in the United States than they are in the same latitudes of Europe. On the parallel of 45°, we find in North America an average of 40 auroras annually; but in Europe less than ten.

Geographical Distribution of Thunder Storms.

The geographical distribution of auroras is believed to be intimately related to the geographical distribution of thunder storms. I have therefore made a considerable collection of facts showing the average annual number of days of lightning at different points of the earth's surface. In the following table, column 4th shows the average annual number of days of lightning for the places named in column 1st. Column 5th shows the number of years embraced in the comparison; and column 6th shows the authority for the statements.

Table VI.—*Average number of days of Lightning annually.*

Place.	Latitude.	Longitude.	No. per year.	Y'rs embraced.	Authority.
	° ′	° ′			
Georgetown, Guiana,	6 49	58 11 W.	60	11	Brit.Guiana Met.Obs.1846–56.
Ethiopia,	11	36 E.	82	5	Astron. Nachricht., 1204, 62.
Lima, Peru,	12 0	77 2 W.	0		Arago Met. Essays, p. 109.
Abyssinia,	13	37 E.	38	1	Arago Met. Essays, p. 129.
Madras,	13 4	80 16 E.	144	5	Madras Obs., 1841–45.
Central Africa,	12 to 18	4E. to 4W.	65	2	Barth's Central Africa.

TABLE VI—*Continued.*

Place.	Latitude.	Longitude.	No. per year.	Y'rs embraced.	Authority.
	° ′	° ′			
Martinique,	14 40	61 3 W.	39		Arago Met. Essays, p. 129.
St. Helena,	15 55	5 54 E.	0	3½	St. Helena Obs., 1840–43.
Guadeloupe,	16 12	61 45 W.	37		Arago Met. Essays, p. 129.
Bombay,	18 50	72 50 E.	42	5	Bombay obs., 1845–49.
Calcutta,	22 35	88 25 E.	60	1	Arago Met. Essays, p. 128.
Rio Janeiro,	23 0 S.	43 14 W.	50·6	6	" " " " "
Patna, India,	25 31	85 10 E.	53	1	" " " " "
Cairo,	30 6	31 26 E.	3·5	2	" " " " 134.
Buenos Ayres,	34 30 S.	58 28 W.	22·5	7	" " " " 130.
Gibraltar,	36 7	5 21 W.	6		Berghaus Annalen, 200, 6.
Athens,	37 58	23 44 E.	11	3	Arago Met. Essays, p. 133.
Palermo,	38 7	13 19 E.	13·5		Kämtz Meteorology, p. 362.
Smyrna, Asia,	38 23	27 11 E.	19	1	Arago Met. Essays, p. 130.
Washington,	38 53	77 0 W.	13·5	3	Gilliss's Met. Observations.
Maryland,	39 0	75 30 W.	41	1	Phil. Trans., 1759, p. 58.
Janina, Turkey,	39 47	20 59 E.	45	10	Kämtz Met., ii, 463.
Pekin, China,	39 53	116 40 E.	5·8	6	Arago Met. Essays, p. 134.
Philadelphia,	39 58	75 10 W.	10	3	Journal Franklin Institute.
New York,	40 42	74 0 W.	25·4	10	N. Y. Regents Rep., 1843–56.
Hudson, Ohio,	41 14	81 25 W.	13	7	Manuscript observations.
North Salem, N. Y.	41 20	73 38 W.	22·7	10	N. Y. Regents Rep., 1843–51.
Litchfield, Conn.,	41 45	73 8 W.	25·0	3	" " " 1851–52.
Amenia, N. Y.,	41 48	73 36 W.	18	1	" " " 1850, 241.
Rome, Italy,	41 54	12 25 E.	42·4	11	Kämtz, Met., ii, 463.
Rodez, France,	42 0	2 39 E.	11	1	Annuaire Met., 1851, p. 122.
Albany, N. Y.,	42 40	73 44 W.	21·6	7	N. Y. Regents Rep., 1840–49.
Marseilles, Fr.,	43 18	5 22 E.	9·3	9	Kämtz Met., ii, 463.
Toulouse, Fr.,	43 36	1 26 E.	17·1	8	Annuaire Met., 1851, 131. Arago Met. Essays, p. 131.
Toronto, C. W.,	43 39	79 21 W.	20	2	N. Y. Regents Rep., 1850, 371.
Lowville, N. Y.,	43 47	75 33 W.	12·7	3	N. Y. Regents Rep., 1840–43.
Newbury, Vt.,	44 6	72 5 W.	8·7	3	" " " 1854–56.
Orange, France,	44 7	5 50 E.	12·2	31	Annuaire Met., 1851, p. 167.
Somerville, N. Y.,	44 10	75 25 W.	17·3	3	N. Y. Regents Rep., 1850–52.
Padua, Italy,	45 24	11 52 E.	41·9	12	Kämtz Met., ii, 463.
La Rochelle, Fr.,	46 10	1 9 W.	21·0	8	Kämtz Met., ii, 455.
Geneva, Switz.,	46 12	6 10 E.	19	4	Gehler, 4, 2, p. 1585.
Bourg, France,	46 12	6 15 E.	37	1	Annuaire Met., 1851, p. 188.
Morges, Switz.,	46 31	6 28 E.	22·8	5	Bibliotheque Universelle, March 1860, p. 229.
Quebec, C. E.,	46 49	71 12 W.	23·3		Arago Met., p. 129.
Dijon, France,	47 19	5 2 E.	16	1	Annuaire Met., 1851, p. 84.
Viviers, France,	47 30	4 40 E.	24·7	10	Arago Met., p. 129.
Ofen, Hungary,	47 30	19 2 E.	28·0	11	Kämtz Met., ii, 459.
Tegernsee, Bav.,	47 30	11 32 E.	23·2	9	" " " 457.
Peissenberg, Bav.,	47 48	11 1 E.	23·0	12	" " " "
St. Andex, Germ.,	47 58	11 12 E.	27·1	12	" " " "
Denainvilliers, Fr.,	48 0	2 20 E.	20·6	24	Arago Met., p. 130.
Munich, Bav.,	48 8	11 35 E.	22·7	12	Kämtz Met., ii, 457.
Vienna, Austria,	48 12	16 20 E.	8·3	20	" " " "
Augsburg, Bav.,	48 21	10 54 E.	22·3	12	" " " "
Tubingen, Würt.,	48 32	9 2 E.	14·6	9	Arago Met., p. 132.
Lugan, Russia,	48 35	39 21 E.	35	1	Berghaus Annalen, 200, 5.
Strasbourg, Fr.,	48 36	7 46 E.	17	20	Arago Met., p. 131.
Giengen, Würt.,	48 37	10 15 E.	21·9	12	Kämtz Met., ii, 456.
Hohenfurth, Boh.,	48 37	17 40 E.	28		Berghaus Annalen, 200, 10.
Stuttgart, Würt.,	48 46	9 10 E.	20·6		Kämtz Met., ii, 456.

TABLE VI.—*Continued.*

Place.	Latitude.	Longitude.	No. per year.	Y'rs embraced.	Authority.
	° ′	° ′			
Versailles, France,	48 48	2 7 E.	15	1	Annuaire Met., 1851, p. 55.
Paris, France,	48 50	2 20 E.	13·6	51	Arago Met., p. 132.
Goersdorff, France,	48 57	7 48 E.	27	1	Annuaire Met., 1851, p. 181.
Regensberg, Bav.,	49 1	11 56 E.	16·9	10	Kämtz Met., ii, 457.
Rehberg, Boh.,	49 6	13 27 E.	29		Berghaus Annalen, 200, 10.
Metz, France,	49 7	6 10 E.	16	1	Annuaire Met., 1851, p. 109.
Tabor, Bohemia,	49 24	16 59 E.	5		Berghaus Annalen, 200, 10.
Rouen, France,	49 26	1 5 E.	13	1	Annuaire Met., 1851, p. 149.
Mannheim, Bav.,	49 28	8 27 E.	20·8	12	Kämtz Met., ii, 456.
Seelau, Boh.,	49 32	17 33 E.	12		Berghaus Annalen, 200, 10.
Brzesnitz, Boh.,	49 34	9 17 E.	27		" " " "
Deutschbrod, Boh.,	49 36	17 55 E.	16		" " " "
Würzburg, Bav.,	49 46	9 54 E.	13·9	8	Kämtz Met., ii, 427.
Brzezina, Poland,	49 49	15 58 E.	27		Berghaus Annalen, 200, 10.
Landskron, Boh.,	49 55	18 57 E.	11		" " " "
Tepel, Bohemia,	49 58	12 52 E.	11		" " " "
La Chapelle, Fr.,	50 0	3 35 E.	15·7	18	Arago Met., p. 131.
Eger, Bohemia,	50 5	14 42 E.	15		Berghaus Annalen, 200, 10.
Prague, Bohemia,	50 6	14 23 E.	17·7	10	Kämtz Met., ii, 457.
Smetschna, Boh.,	50 11	16 22 E.	11		Berghaus Annalen, 200, 10.
Königgrätz, Boh.,	50 13	18 10 E.	28		" " " "
Polpero, Eng.,	50 20	4 10 W.	10	23	Arago Met., p. 133.
Oberwiesenthal, Boh.	50 25	12 58 E.	15		Berghaus Annalen, 200, 10.
Hohenelbe, Boh.,	50 38	17 54 E.	20		" " " "
Altenberg, Boh.,	50 45	13 43 E.	15		" " " "
Tetschen, Boh.,	50 47	16 32 E.	17		" " " "
Brussels, Belg.,	50 50	4 30 E.	16·1	8	Kämtz Met., ii, 455.
Maestricht, Hol.,	50 51	5 42 E.	16·5	11	Arago Met., p. 131.
Zittau, Sax.,	50 54	14 48 E.	19		Berghaus Annalen, 200, 10.
Freiberg, Sax.,	50 55	13 20 E.	13		" " " "
Erfurt, Prussia,	50 58	11 2 E.	14·1	8	Kämtz Met., ii, 457.
Schluckenau, Boh.,	51 1	14 27 E.	14		Berghaus Annalen, 200, 10.
Dresden, Sax.,	51 3	13 44 E.	20		" " " "
Nertschinsk, Rus.,	51 18	119 20 E.	3·1	6	Kämtz Met., ii, 459.
Middleburg, Belg.,	51 28	3 37 E.	21·3	6½	" " " 455.
London, Eng.,	51 30	0 7 W.	8·3	13	Arago Met., p. 133.
Sagan, Prussia,	51 37	15 19 E.	29·3	12	Kämtz Met., ii, 457.
Münster, Pr.,	51 58	7 38 E.	29·7		Berghaus Annalen, 200, 5.
Utrecht, Hol.,	52 6	5 8 E.	15		Arago Met., p. 132.
Leyden, Hol.,	52 9	4 30 E.	13·5	29	" " " 133.
Irkutsk, Rus.,	52 17	104 17 E.	8·5	2	Kämtz Met., ii, 459.
Minden, Pr.,	52 18	8 55 E.	5·8		Berghaus Annalen, 200, 5.
Berlin, Prussia,	52 31	13 21 E.	17·3	120	Kämtz Met., ii, 457.
Lüneberg, Han.,	53 14	10 28 E.	20·2	20	" " " 456.
Barnaul, Russia,	53 20	83 27 E.	24	1	Berghaus Annalen, 200, 6.
Hamburg,	53 33	9 58 E.	10·7		Kämtz Met., ii, 456.
Cuxhaven, Germ.,	53 53	8 44 E.	11·5	10	" " " "
Braunsberg, Pr.,	54 20	19 54 E.	30·7		Berghaus Annalen, 200, 5.
Apenrade, Den.,	55 3	9 25 E.	19		" " " "
Tilsit, Prussia,	55 5	21 45 E.	12·6		" " " "
Slatoust, Russia,	55 11	59 45 E.	14	2	" " " "
Copenhagen, Den.,	55 41	12 35 E.	1	4	Gehler Wört., v. 4, 2, p. 1585.
Memel, Prussia,	55 42	21 6 E.	4	26	Berghaus Annalen, 200, 5.
Moscow, Russia,	55 47	37 44 E.	22·4	9	Kämtz Met., ii, 459.
Kasan, Russia,	55 52	49 30 E.	9	1	" " " "
Jekaterinenburg, Rus.	56 50	60 34 E.	23·3	3	Berghaus Annalen, 200, 5.

TABLE VI.—*Continued.*

Place.	Latitude.	Longitude.	No. per year.	Y'rs embraced.	Authority.
	° ′	° ′			
Skara, Sweden,	58 22	12 28 E.	9·2	25	Kämtz Met., ii, 461.
Stockholm, Sweden,	59 21	18 4 E.	9·3	10	" " " "
Spydberga,	59 38	9 E.	7·7	3	" " " "
Bogoslowsk, Rus.,	59 45	59 59 E.	2	1	Berghaus Annalen, 200, 6.
Petersburg, Rus.,	59 52	30 25 E.	12·4	10	Kämtz Met., ii, 459; Arago.
Abo, Russia,	60 15	22 10 E.	10	12	" " " "
Bergen, Nor.,	60 24	5 18 E.	5·8	7	" " " 461.
Sondmör, Nor.,	62 30	6 20 E.	3·9	12	" " " "
Archangel, Rus.,	64 32	40 33 E.	6·5	18	Berghaus Annalen, 200, 5.
Reikiavik, Iceland,	64 8	22 0 W.	½	2	Arago Met., p. 111.
Fort Franklin,	65 12	123 12 W.	1	2	" " " "
Baffin's Bay,	65 30	80 W.	1	1	" " " 110.
Melville Island,	74 47	110 49 W.	0	2	" " " "
At sea,	75	100 W.	0	1	" " " "

It is obvious from an examination of the preceding table, that thunder storms prevail most frequently in the equatorial regions and diminish as we proceed towards the poles. It is also evident that the frequency of thunder showers depends upon other circumstances than simply latitude; but without stopping to enquire what these circumstances are, we will take the average of the observations included between different parallels of latitude. We thus find that:—

between lat. 0° and lat. 30°	the average number of thunder storms annually is	51·6
" " 30 " " 50		19·9
" " 50 " " 60		14·9
" " 60 " " 70		4·0
beyond " 70		0·0

Maury's storm and rain charts of the Atlantic Ocean furnish most important information on the same subject. The following table presents a summary of the results of these charts. The ocean is divided into squares by parallels of latitude drawn at intervals of five degrees from each other, and meridians of longitude at intervals of five degrees. Each square of the following table contains three numbers. The first shows the number of days of observation within the given square; the second shows the number of days of lightning reported, and the third is the number of days of lightning which would occur in one year, as computed from the numbers actually observed. Thus in the square included between the parallels of 30 and 35 degrees of north latitude, and between the meridians of 65° and 70°, the first number is 548, which shows that 1644 observations have been obtained in that square. Each observation represents a period of 8 hours, so that 1644 observations represent 548 days. During this period, lightning was reported on 44 days, which is at the rate of 29 cases for one year.

TABLE VII.—*Days of Lightning in the Atlantic Ocean, by Maury's charts.*

Lat.	80°–75°	75°–70°	70°–65°	65°–60°	60°–55°	55°–50°	50°–45°	45°–40°	40°–35°	35°–30°	30°–25°	25°–20°	20°–15°	15°–10°	10°–5° W.	5° W.–0°	0°–5° E.	5° E.–10°	10°–15°	15°–20°
60°–55											20 0 0	34 1 11	41 0 0	39 1 9	26 0 0	10 0 0				
55–50								50 0 0	140 2 5	170 1 2	231 6 9	283 6 8	311 2 2	420 5 4	464 4 3	194 4 7				
50–45					42 1 9	96 0 0	306 5 6	414 5 4	523 7 5	579 11 7	542 8 5	513 11 8	493 5 4	437 5 4	30 2 2	104 0 0				
45–40		607 11 7	1083 48 16	824 56 25	893 46 19	806 37 17	621 24 14	527 23 16	373 14 14	244 5 8	132 5 14	90 0 0	56 0 0	43 0 0	22 0 0					
40–35	81 1 5	1398 70 18	991 81 29	599 40 24	464 24 19	370 22 22	248 12 18	160 3 7	116 1 3	81 0 0	114 7 22	89 3 12	101 0 0	113 1 3	111 0 0	75 0 0				
35–30	511 41 29	755 42 20	548 44 29	255 7 10	241 17 26	287 8 10	329 11 12	298 13 16	249 17 25	131 2 5	58 0 0	43 1 8	78 1 5	26 0 0	1 0 0					
30–25	648 33 19	464 15 12	379 24 23	316 23 27	131 12 33	117 7 22	188 6 12	242 4 6	319 29 33	221 6 10	70 2 10	51 0 0	29 0 0							
25–20	105 5 17	120 6 17	87 1 4	217 8 14	212 2 4	89 1 4	87 0 0	151 2 4	268 8 11	221 2 3	113 0 0	45 0 0	5 0 0							
20–15	81 2 9	107 9 31	51 2 14	61 5 30	153 1 2	180 3 6	109 3 10	101 3 11	150 0 0	237 5 8	213 0 0	53 1 7	2 0 0							
15–10	22 0 0	0 0 0	18 0 0	18 0 0	32 0 0	129 4 11	198 4 7	169 18 39	138 9 24	222 13 21	278 2 3	207 3 5	75 3 15							
10–5 N.							96 1 4	223 6 10	211 16 28	246 14 21	556 42 27	336 12 13	161 1 2	239 6 9	27 0 0	23 0 0	18 0 0			
5 N.–0							32 0 0	140 8 21	204 4 7	335 13 14	668 40 22	421 32 28	112 6 19	36 3 30	121 3 9	78 9 42	33 1 11	74 3 15		

5 S.								28 0 0	545 11 7	817 13 6	1182 15 5	680 12 6	193 3 6	30 0 0	20 0 0	5 0 0	9 0 0	4 0 0		
10									91 0 0	1365 12 3	733 2 1	558 0 0	383 0 0	100 0 0	6 0 0	2 0 0	6 0 0	5 0 0	8 0 0	
15									337 5 5	1260 5 1	481 0 0	138 0 0	266 0 0	479 0 0	251 0 0	11 0 0	5 0 0			
20									1314 14 4	1171 5 2	604 1 1	121 1 3	51 0 0	101 0 0	457 0 0	548 0 0	64 0 0	13 0 0		
25								438 21 17	1108 28 9	930 16 6	858 5 2	257 2 3	65 0 0	23 0 0	52 3 20	287 0 0	523 0 0	121 0 0	10 0 0	
30							295 13 16	731 37 18	741 30 15	706 13 7	597 12 7	400 1 1	198 1 2	71 0 0	47 0 0	77 0 0	154 0 0	523 4 3	187 0 0	5 0 0
35						279 29 38	802 26 12	799 27 12	577 16 10	416 19 17	334 2 2	552 4 3	502 6 4	392 1 1	299 0 0	280 1 1	299 1 1	656 2 1	1077 5 2	500 9 6
40					168 19 41	682 37 20	1119 67 22	773 9 4	495 9 7	218 0 0	348 0 0	715 1 1	523 8 6	688 6 3	695 0 0	876 2 1	814 0 0	818 0 0	619 1 1	992 5 2
45				268 7 10	727 33 17	750 36 17	658 9 5	515 10 7	157 2 5	33 0 0	18 0 0	33 0 0	3 0 0	33 0 0	96 0 0	292 0 0	127 0 0	163 0 0	53 0 0	47 0 0
50			66 1 6	692 27 14	595 10 6	466 10 8	311 1 1	55 0 0	10 0 0	6 0 0	0	0	0	0	2 0 0	2 0 0	0	1 0 0	1 0 0	1 0 0
55			236 2 8	927 11 4	560 2 1	316 0 0	36 0 0	3 0 0	3 0 0	0	0	0	0	0	0	0	0	0	0	0
60			1469 15 4	866 11 5	135 0 0	9 0 0	0	0	0	0	0	0	0	0	0	0	0	0	0	0

We see in this table abundant evidence that the frequency of lightning depends upon other circumstances than simply latitude. Throughout the western half of the Atlantic Ocean, lightning occurs *three times* as frequently as throughout the eastern half of that ocean. If we take the average of all the observations upon the same parallel of latitude, we shall find the number of days of lightning to be as follows:

	North Atlantic.	South Atlantic.
From lat. 0 to lat. 5°	20	6
" " 5 " 10	17	2
" " 10 " 15	14	1
" " 15 " 20	8	2
" " 20 " 25	7	6
" " 25 " 30	19	9
" " 30 " 35	19	7
" " 35 " 40	19	6
" " 40 " 45	15	9
" " 45 " 50	5	8
" " 50 " 55	5	3
" " 55 " 60	4	4

The average frequency of lightning in the North Atlantic is *two and a half times* as great as in the South Atlantic; also the average frequency of lightning on land, at least in the northern hemisphere, appears to be *more than twice* as great as in the North Atlantic. The bearing of the preceding facts upon the theory of auroras will be considered hereafter.

Art. XII.—*On the Products of the Distillation of Common Rosin;* by James Schiel, of St. Louis.

The chemical process taking place in the distillation of common rosin or colophony, and the two different liquids which are obtained by that distillation, have as yet escaped the attention of chemists; the whole process is almost completely wrapped in darkness. The two liquids just mentioned are distinguished as *essence of rosin* (in Europe "German oil of turpentine") and *rosin oil;* the former is thin, of a light yellow color, and a strong, almost aromatic odor; the latter is a thick liquid of a somewhat disagreeable odor.

The *essence of rosin* is a mixture of two substances, which may be separated by fractional distillation if often repeated. During this distillation the liquid passing over sometimes assumes a milky aspect, produced by the formation of a small portion of water and a trace of acetic acid; a small piece of lime or baryta instantly renders the liquid perfectly transparent. Of the two liquids into which the essence of rosin separates, the first is very thin, perfectly colorless and transparent, and of a strong, refracting power. I call it *colophonon.* The specific gravity of

colophonon is 0·84 at 14° C., its boiling point 97° C. (Barom. ·756); with concentrated sulphuric acid it forms a brown liquid from which water separates a green oil, having much the smell of Ol. anthos. Chlorhydric acid produces a similar transformation. With potassium it gives rise to a lively emission of gas and coagulates into a brown-yellow mass. Heated in a closed vessel above the boiling point it colors brown and assumes an odor of peppermint.

The composition of colophonon is expressed by the formula

$$C_{11}H_{18}O_2,$$

One hundred parts consist of

	Calculated.	Found.		
Carbon,	79·52	78·90	78·95	79·50
Hydrogen,	11·85	11·59	11·61	12·01
Oxygen,	9·63	9·51	9·44	9·49
	100·00	100·00	100·00	100·00

The determination of the specific gravity of (vapor of) colophonon gave the number 5·1, which however I consider merely as an approximation. It is remarkable, that this substance, whose composition is distinguished from that of *phoron* by C_2H_4 should have a boiling point as low.

The second constituent of the essence of rosin has its boiling point at 160° C., and possesses all the chemical properties of oil of turpentine. The analysis of it showed it to be composed of

Carbon,	87·44
Hydrogen,	11·78

The formula of oil of turpentine $C_{10}H_{16}$ requires

Carbon,	88·23
Hydrogen,	11·77
	100·00

As it does not seem to have any effect on the plane of polarization, it may be identified with the *therebène* of Deville.

The oil of rosin, with a boiling point above that of mercury, does not seem to have a constant composition; at least, there is sometimes a difference between the raw oil and the refined oil. The raw oil, which has the property of fluorescence, loses this property in a great degree by a very simple refining process. This process consists simply in placing the earthen vessels containing the raw oil on the hot wall around the neck of the iron retort, from which the rosin is distilled, and allowing them to stand there for some hours, keeping them covered up. A portion of the raw oil that had been heated in the water-bath in contact with caustic lime showed the composition $C_{20}H_{28}O_2$ of the *resineine* of Deville and Frémy, viz:

	Calculated.	Found.
Carbon,	84·51	84·70
Hydrogen,	9·80	9·69
Oxygen,	5·69	5·61
	100·00	100·00

A portion of the refined oil, treated in the same way, gave the formula $C_{15}H_{20}O_2$, viz:

Carbon,	86·44	86·25
Hydrogen,	9·62	9.95
Oxygen,	2·94	2·77
	100·00	100·00

Further investigation will show whether this difference is purely accidental, and there is good reason to believe it to be so; or whether there is really a difference in the composition of the two products.

The gases formed during the distillation of the rosin burn with a bright light and may be used for illumination. I have analyzed a portion of these gases, collected in glass tubes which were sealed up with the blowpipe, in the gas-room of Prof. Bunsen. They contain 14·96 p. c. carbonic acid, 11·45 p. c. of oxyd of carbon, 5·89 p. c. ethylene and butylene (ditetryl), besides oxygenated nitrogen very nearly in the proportion of 1 : 6. Towards the close of the distillation the oxygen nearly disappeared, and a portion of light carburetted hydrogen made its appearance.

I have to remark that it must be left undecided whether there may not be an admixture of some propylene with the above 5·89 parts of ethylen and ditetryl, as we do not yet possess any means of analyzing these homologous gases. The analysis of the three homologous gases, ethylene, propylene and butylene, would furnish the equations:

$$x+y+z=V$$
$$2x+3y+4z=B$$
$$2x+2{\cdot}5y+3z=C$$

V being the volume analyzed, B the volume of carbonic acid formed by the combustion, and C the contraction. From the first of these equations we have $x=V-z-y$, and this value of x introduced into the two other equations, makes them

$$y+2z=B-2V$$
$$2y+4z=C-2V$$

The second of these is evidently the half of the first and $C=\frac{1}{2}B$. For the determination of y and z we therefore have only *one* equation. If there is a large quantity of the three gases to be disposed of, they may be absorbed by sulphuric acid and the alcohols of the two first may be distilled off after dilution with water.

By sending a current of common coal-gas slowly for an entire week through a number of flasks containing sulphuric acid, then diluting with water and distilling; I found on the surface of the water distilled over, a light oily substance, collecting in large drops, and having a strong, disagreeable odor. It is very likely that to this substance dissolved in the odorless parts of the gas is due the offensive odor of coal-gas. I hope yet to obtain enough of this oily substance for an analysis.

Heidelberg, May 1, 1860.

ART. XIII.—(1.) *An account of the fall of Meteoric Stones at New Concord, Ohio, May 1st,* 1860; by Prof. E. B. ANDREWS, of Marietta College. With (2.) *Computations respecting the Meteor;* by Prof. E. W. EVANS, of the same Institution. *To which are added further notices of the same by* D. W. JOHNSON, Esq. and Dr. J. LAWRENCE SMITH.

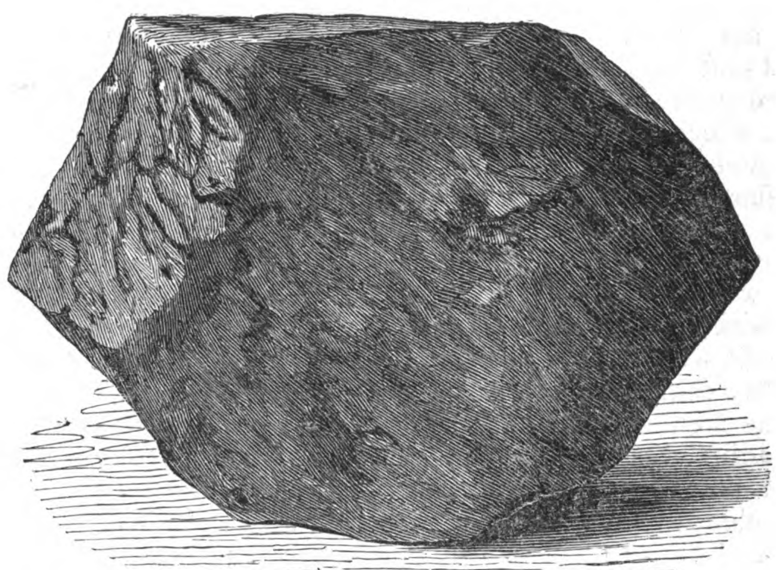
(1.)

ABOUT fifteen minutes before one o'clock on the first day of May, 1860, the people of southeastern Ohio and northwestern Virginia were startled by a loud noise, which was variously attributed to the firing of a heavy cannon, to the explosion of steamboat boilers, to an earthquake, and to the explosion of a meteor. In many cases houses were jarred. To persons within doors the noise generally seemed as if produced by the falling of a heavy soft body upon the chamber floor. Many persons heard a rumbling reverberation which continued for a few seconds. The area over which this explosion was heard was probably not less than one hundred and fifty miles in diameter. At Marietta, O., the sound came from a point north or a little east of north. The direction of the sound varied with the locality. An examination of all the different directions leads to the conclusion that the central point, from which the sound emanated, was near the southern part of Noble county, Ohio.

At New Concord, Muskingum Co., where the meteoric stones fell, and in the immediate neighborhood, there were many distinct and loud reports heard. At New Concord there was first heard in the sky, a little southeast of the zenith, a loud detonation, which was compared to that of a cannon fired at the distance of half a mile. After an interval of ten seconds another similar report. After two or three seconds another, and so on with diminishing intervals. Twenty-three distinct detonations were heard, after which the sounds became blended together and were compared to the rattling fire of an awkward squad of soldiers, and by others to the roar of a railway train. These sounds, with their reverberations, are thought to have continued for two minutes. The last sounds seemed to come from a point in the southeast 45° below the zenith. The result of this cannonading was the falling of a large number of stony meteorites upon an area of about ten miles long by three wide. The sky was cloudy, but some of the stones were seen first as "black specks," then as "black birds," and finally falling to the ground. A few were picked up within twenty or thirty minutes. The warmest was no warmer than if it had lain on the ground exposed to the sun's rays. They penetrated the earth from two to three feet. The largest stone, which weighed 103 lbs., struck the earth at the foot of a large oak tree, and after cutting off two roots, one five inches in diameter, and grazing a third root,

it descended two feet ten inches into hard clay. This stone was found resting under a root which was not cut off. This would seemingly imply that it entered the earth obliquely. It is said that other stones which fell in soft ground entered the earth at a similar angle. They must have been flying in a northwest direction. This fact, added to the other facts, that the detonations heard at New Concord came lower and lower from the zenith toward the southeast, and that the area upon which the stones fell extends with its longer axis in a southeast and northwest direction, would imply that the orbit of the meteor, of which these stones are fragments, extended from southeast to northwest. This conclusion is confirmed by the many witnesses who saw, at the time, a luminous body moving in the same direction. It is a fact of some interest that the larger stones were carried by the orbital force further than the small ones, and were found scattered upon the northwest end of the area referred to. This fact is readily explained by the larger proportional surface presented to the atmospheric resistance in the smaller stones. The stones thus far found vary in weight from a few ounces to over a hundred pounds. They show a decided family resemblance. All are coated with a black crust and show a bluish gray feldspathic interior with numerous brilliant points of nickeliferous iron. Although in some instances the edges remain quite sharply

1.



defined, generally they show that they have been rounded by fusion. The accompanying figure shows the appearance of the larger stone now in the cabinet of Marietta College. Viewed from most positions this stone is angular and appears to have

been recently broken from a larger body. On one side it is much rounder and smoother, and this (the outer surface in the figure) appears to be a part of the original surface of the main meteor. Two of its edges extend more than a foot in length, and two of its diameters are fourteen inches. In the small stones the edges are, I think, more rounded than in the larger ones. The angle at A, fig. 2, is an exact copy of a specimen in my possession. The dotted line shows the thickness of the crust.

2.

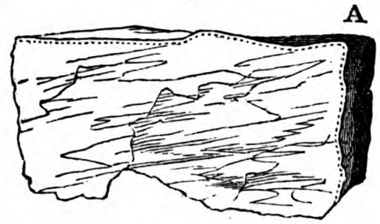
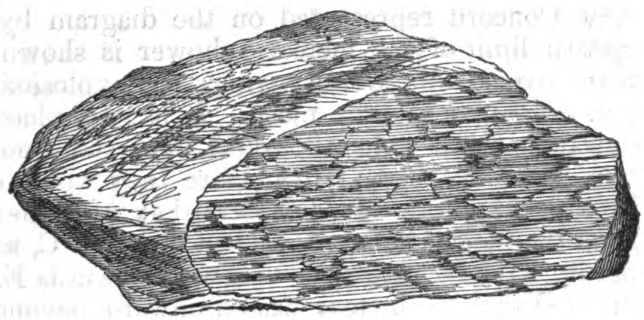


Figure 3 represents the appearance of a small stone, one side of which shows a surface only partially glazed. There was evidently a flaw in this little meteorite, and the heat entering the crack was only sufficient to fuse the surface in a very slight degree. The heat apparently penetrated the crack in straight lines as if driven backward by the high velocity. The edge of the stone surrounding this peculiar surface is a feather edge made by the melting of the metallic crust in an unusual manner.

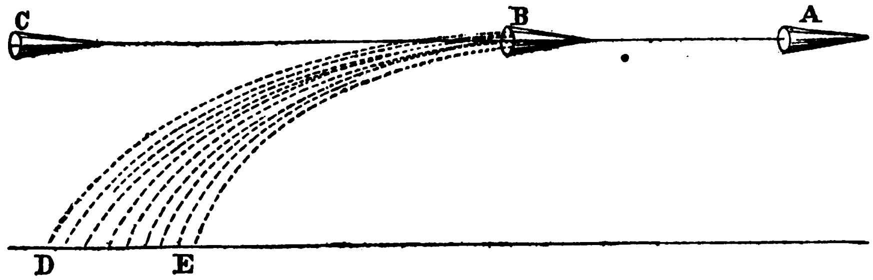
3.



In the examination of this interesting meteoric phenomenon, I am led to believe that the people of New Concord and in the immediate vicinity of the district where the stones fell, heard different sounds, and consequently of different origin, from those heard by people living at a greater distance. The former heard many distinct detonations followed by a rumbling roar like that of thunder. The latter heard but a single explosion followed by a somewhat similar rumbling noise but less distinct. This explosion seemed to take place at a point in the air over the southern part of Noble County. The people of the northern part of the same county heard it in a southern or southeastern direction, and *not in a northwestern direction towards New Concord.* This fact would indicate that the great explosion which was heard more than seventy-five miles away, took place in Noble County, and that the several distinct detonations heard at and near New Concord were directly connected with the falling of the several stones in that district. A diagram will illustrate this and also one or two other points.

By the careful computations of Prof. Evans the meteor passed in its orbit from the point A over Newport, Washington Co.,

4.



Ohio, to the point C, which is quite as far to the northwest as New Concord represented on the diagram by D. The south-eastern limit of the meteoric shower is shown by E. B is the point over Noble County where the explosion was almost universally *heard* to take place. If it took place there, then the onward orbital velocity would take the stones forward from twenty-five to thirty miles before they would drop to the earth between D and E. It is evident that the stones were thrown off from the meteor before it reached the point C, as the stones could hardly have been thrown backward towards E. It is also to be inferred that the meteor passed onward beyond C, although the clouds prevented further observations in that direction. The detonations heard between D and E must be accounted for by the rapid passing of the several stones through the lower atmosphere.

(2.) *Prof. Evans's Computations.*

Owing to the cloudy state of the atmosphere, the time was unfavorable for observing such facts as are necessary for the accurate determination of the height of the meteor, the direction of its path, its size and its velocity. After careful investigation, however, the following results have been obtained.

1. *Direction of its path.*—The district along which the meteorites are known to have fallen is about ten miles long and from two to three miles wide, extending in a northwesterly direction from a little west of the village of Point Pleasant, in Guernsey County, to within a mile of New Concord, in Muskingum Co.* The fragments fell with a northwesterly inclination. This is proved both by the testimony of those who saw them descend, and by the direction in which they were subsequently found to have penetrated the earth. As the sky along this district was overcast with clouds, the main body of the meteor was not seen

* As nearly as can be made out from the data, the path of the meteor appears to have crossed the Ohio River in lat. 39° 30′, long. 81° 20′, and to have disappeared in lat. 40° 2′, long. 81° 90′.—Eds.

by those who witnessed the fall of the fragments; but the sounds, as heard by them, first proceeded from the zenith, and gradually receded towards the southeast. This seemingly contradictory fact agrees perfectly with the hypothesis that the course of the meteor was northwesterly; for if it approached with a velocity greatly exceeding the velocity of sound, the explosions which occurred last must have been the first heard. At some distant stations towards the south and west the view was not wholly obstructed by clouds; and there are many witnesses who relate that a few minutes before any report was heard, they saw a large ball of fire pass across the northern sky, towards the northwest. By tracing out the line along which the reports were loudest and seemed to proceed from the zenith, I am led to the conclusion that the meteor passed over the eastern end of Washington County, then across the interior of Noble County, then over the southwestern corner of Guernsey and the northeastern corner of Muskingum, with a direction of about 42° west of north.

2. *Its height above the earth.*—Mr. William C. Welles of Parkersburg, Virginia (lat. 39° 10′, long. 81° 24′), a gentleman of liberal education, testifies that being about three miles east of that place at the time of the occurrence, he happened to look up to the northeast of him, and saw a meteor of great size and brilliancy, emerging from behind one cloud and disappearing behind another. When about 35° east of north he thinks its altitude was 65°. Now the distance, in a direction 35° east of north, from his station to the line directly under the meteor's path, is 20 miles. Calculating from these data I find for the vertical height, taken to the nearest unit, 43 miles. This was at a point in Washington County near the border of Noble.

Mr. C. Hackley testifies that he saw the meteor from Berlin in Jackson County. It crossed a cloudless space in the northeast, and he thinks its altitude, at the highest point, was 30°. Now the distance from Berlin to the nearest point under the meteor's path is 70 miles. These data give nearly 41 miles for its vertical height over Noble County, a few miles to the south of Sarahsville (lat. 39° 53′, long. 81° 40′).

Many other reliable witnesses have been found who saw the meteor through openings in the clouds from various points west of its path; and whose testimony so far agrees with the foregoing as to give results ranging between 37 and 44 miles. Care has been taken as far as possible to verify the data in each case by personal examination of the witnesses. The angles have in most instances been taken as pointed out by them from their respective posts of observation. It is unfortunate that no case has come to our knowledge in which the meteor was seen from the region east of its path. But it was a circumstance in some

respects favorable to the definiteness of the observations made from the west side, that the observers in nearly all cases saw the meteor only at one point, or within a very small space on the heavens. It is impossible to reconcile the various accounts without granting that its path was very nearly as above described, and that its height did not vary far from 40 miles as it crossed Noble County.

In regard to the time which intervened, at different places, between seeing the fire-ball and hearing the report, the statements are so vague that not much reliance has been placed upon them. It may be remarked, however, that they will essentially agree with the foregoing conclusions, if we suppose that the loudest explosion took place in the southern part of Noble County.

I will add under this head the statement of Mr. Joel Richardson, of Warren, Washington County, who from a place six miles west of Marietta, saw the meteor as much as 15° or 20° west of north, at an altitude of about 45°. The direction in this case was so oblique to the meteor's path, that the data are of little value for simply determining the height; but they are important on account of their connection with the place of the meteor's last appearance. Mr. Richardson was visited by the writer, and his testimony was subjected to close scrutiny. If we take the azimuth at 15° west of north, we shall have a distance of 41 miles to the line under the meteor's path; and these data will give about 41 miles for its vertical height over a point not more than a mile from New Concord, at the extreme western limit of the district along which the meteorites were scattered. If we take the azimuth at 20° west of north, both the distance and the height will be greatly augmented. I have found two persons living near Bear Creek, nine miles north ot Marietta, who make statements closely corroborating that of Mr. Richardson.

3. *Velocity of the meteor.*—Mr. Welles furnishes data from which we can now determine approximately the meteor's rate of motion. As this gentleman is somewhat accustomed to astronomical observation, his judgment as to angles may be strongly relied upon. He thinks he saw the meteor pass from a point 50° east of north to a point 20° east of north in about three seconds. These two points in the meteor's path are over the townships of Newport in Washington County, and Elk in Noble County. The distance between them is 12 miles. According to these data, then, its relative velocity was about four miles a second. No other statement regarding the velocity has been obtained that is sufficiently definite to be of any value.

4. *Its size and shape.*—Those who saw the fire-ball from stations not less than 20 and not more than 30 miles to the westward, agree in stating that it appeared as large and as round as

the full moon. Its intense brilliancy may have produced exaggerated conceptions of its size. But if we take the minimum apparent diameter of the moon, and the minimum distance of the meteor (its height being assumed as 40 miles) we shall have for its diameter 38-hundredths, or about three-eighths of a mile.

The train is described as a cone, having its base upon the fire-ball. As seen from near Parkersburg its length was estimated at twelve times the diameter of the ball. The part next the base appeared as a white flame; but not so bright as to render the outline of the ball indistinct. About half way toward the apex it faded into a steel blue.

(3.) *Notice of the Fall from* D. M. JOHNSON, Esq., *of Coshocton, O.*

[Mr. Johnson's notice of this shower of meteoric stones is the result of a visit to the locality a few days after the event. We copy the following particulars from Mr. Johnson's account as giving circumstantial detail and historic interest to the facts recounted by Profs. Andrews and Evans. Mr. Johnson also adds a chemical analysis of the meteor.—EDS.]

Two carpenters, Samuel L. Hines and Samuel M. Noble, were at work near the house on the farm of Jonas Amspoker, of New Concord. Upon hearing the first report they looked up and saw two dark looking objects, apparently about the size of an apple, come through a cloud, producing a twirling motion in the vapor of the cloud. One of them they saw fall to the ground about one hundred and fifty yards from where they stood. The other one passed behind the house out of their sight. They went immediately to the one which they saw strike the ground, and found it at the bottom of a hole two feet deep. When taken out it was still warm and in a few seconds dried the moist earth adhering to its surface. It was found to weigh 51 pounds.

Nathanael Hines, who was ploughing in a field adjoining Mr. Amspoker's place, heard a report like the blasting of rocks in a well, followed by several smaller reports. He looked up and saw a black body descending to the earth at an angle of about 30° to the vertical. It struck the ground about two hundred yards from him. Repairing to the place he found that in its descent it came in contact with the corner of a fence, breaking off the ends of the three lower rails, and entering the ground about eighteen inches. It was warm, and had a sulphurous smell. This stone was not weighed, but it is estimated to have been between 40 and 50 pounds in weight before any portions were broken off from it. This was probably the stone that the carpenters saw but lost sight of when it passed behind the house.

James M. Reasner was in his house at the time of the explosion, but hearing a noise like striking against the door with the fist he went out, when his attention was attracted by a whizzing

sound over head. Looking up he saw what appeared to be a black streak descending in a slanting direction towards the earth. After he heard that stones had fallen in that vicinity he sought for and found a stone weighing 36½ pounds.

Wm. Law was in his house, one mile east of Concord. Upon hearing the first report he went out into the yard. He heard a buzzing sound passing over the house in a northwest direction, and saw the sheep running in a field not far from the house. Hearing that stones had fallen he went to the field in which the sheep were and found a stone weighing 53 pounds. It had fallen upon the end of a partially decayed log, through which it passed and buried itself it the ground. This was the largest stone that had been found at the time I visited the district. But I since learn that the one described by Prof. Andrews was found after my visit to the place.

A *blazing* meteor was seen from other parts of the state on the same day. The Columbus Statesman of May 5th says that "near McConnellsville several boys observed a huge stone descend to the earth, which they averred looked like a red ball, leaving a line of smoke in its wake." McConnellsville is 25 miles south of Concord.

Mr. D. Mackley of Jackson Co., in a communication to the Cincinnati Commercial, says, "On the first day of May, at precisely half past 12 o'clock, I was standing on the platform at the railroad station in Berlin when I saw, in a northeast direction, a ball of fire, about 30° above the horizon. It was flying in a northerly direction with great velocity. It appeared as white as melted iron, and left a bright streak of fire behind it which soon faded into a white vapor. This remained more than a minute, when it became crooked and disappeared." Berlin is about 80 miles southwest of Concord.

The meteor seen from McConnellsville and Berlin was undoubtedly the same that exploded and fell in Guernsey County. No one of the many persons who saw the stones fall and were in the immediate vicinity at the time, noticed anything of the luminous appearance described by those who saw it from a distance.

All the stones that I have yet seen have the same general appearance. They are irregular blocks, and are covered with a very thin black crust, which looks as if it had been fused. The edges of the blocks are not sharp but rounded, and the faces present the usual pitted appearance of meteorites. They absorb water with a hissing sound. The fragments are of a bluish gray color. Under the lens five substances can be detected. A snow-white mineral is largely disseminated throughout the mass. A clearer white mineral can be distinguished in some specimens. Metallic grains are quite numerous, a yellowish brown mineral

in patches, and black particles scattered over the surface. One specimen had very thin veins of a shining black mineral. When in large masses the stone is exceedingly tough, requiring repeated blows with a hammer to fracture it, but when broken into small pieces it can be crushed with ease in an agate mortar.

The specific gravity of the mass was found to be 3·5417. On analysis one gramme of the stone was found to contain:—

Silica,	·51250
Protoxyd of iron,	·25204
Magnesia,	·08873
Alumina,	·05325
Lime,	·00785
Iron,	·08803
Nickel,	·02360
Sulphur,	·01184
Chromium,	*trace.*
Phosphorus,	*trace.*
Water,	·00035
	1·03819

Coshocton, Ohio, June 4th, 1860.

(4.) Prof. J. LAWRENCE SMITH, M.D., of *Louisville, Ky.*, informs us that, on hearing vague rumors of the event two days after its occurrence (reported as an earthquake accompanied by a fall of stones), and although four hundred miles distant from the place, he immediately visited New Concord and obtained all possible data respecting the phenomenon. He is convinced from a thorough examination of the facts that no fall of meteoric stones before recorded possesses so many points of interest as the one in question, surpassing even the far famed fall at *L'Aigle.* He reserves many details of the event with his chemical examination for a paper in the next number of this Journal. The analyses, so far as they are complete, show the constitution of the New Concord stones to be identical with those that fell about the same time last year, March 28, 1859, in Indiana (see this Jour., xxviii, 409).

Louisville, Ky., June 6, 1860.

Thus far about thirty stones have been recovered from this fall, and one witness estimates the entire weight of the fragments discovered at about seven hundred pounds.

The fine specimen (figure 1) secured by Prof. Andrews for Marietta College, is, we believe, the largest meteoric fragment hitherto recorded as existing in one piece. Profs. Silliman and Kingsley estimated the weight of a fragment of the Weston meteorite (1807), which was dashed in many pieces by falling on a rock, as about 200 pounds.—Mem. Conn. Acad, i, 149.

Art. XIV.—*Review of Dr. Antisell's Work on Photogenic Oils, &c.*

[The following Review of Dr. Antisell's book on Photogenic Oils has been for some months in type waiting an opportunity when our other engagements would permit its publication. It will amply repay the careful perusal of all who are interested in this important practical subject.—Eds.]

REVIEW.

1. *The Manufacture of Photogenic or Hydro-Carbon Oils from Coal and other Bituminous Substances capable of supplying Burning Fluids;* by Thomas Antisell, M.D., Professor of Chemistry in the Medical Department of Georgetown College, D. C., etc. etc. New York and London: D. Appleton & Co. 1859. pp. 144.—In entering an earnest protest against the work before us, we would not have our motives misunderstood. We are not of those who would condemn a book solely on the ground that it is "not so good as it should be," and will not therefore urge this objection against the effort of our Author, although it would be hard to find a case to which the charge would more forcibly apply. But we do condemn most heartily the presumption of the man who in these days attempts to write a handbook upon any scientific or technological subject with which he is not somewhat familiar. We believe, moreover, that errors, either of omission or of commission—accidental or intentional—in scientific writings, which exceed the well-understood conventional limits of *tolerance*, should not be allowed quietly to pass without correction.

Dr. Antisell, from his position of chemical examiner in the Patent Office at Washington, has naturally had a rare opportunity of familiarising himself with the recent improvements which have been made—or claimed—in the manufacture of coal oils. In the work in question, he has published an index of these, which cannot but be acceptable to all who are interested either in the practical or scientific consideration of the subject. Had this list been published by itself, or had it been incorporated with a portion of the materials which Dr. A. has now exhibited, in an article, or a short series of articles, in some one of our scientific or technological magazines, it would have been most gratefully received, and, we doubt not, widely copied. Diluted and scattered as this information has been, however, that it might fill a volume, its value has been lessened in no slight degree.

We have endeavored, in vain, to make out the point of view from which the Author regarded his subject. Claiming the attention of all persons engaged in the manufacture of liquid products from the distillation of mineral combustibles, his work is nevertheless not a didactic one. In it scarcely any attempt is made to instruct the manufacturer, either by a clear enunciation of general principles to be followed, or of special details to be observed in given cases;* while a most lamentable lack of familiarity with the chemistry of the subject is continually exhibited throughout the work. Indeed the book is simply a jumble of badly selected extracts, huddled together in a manner which must be anything but edifying to the student. As a compilation, it has the merit of directing attention to a number of sources from which valuable information may be derived; while it has the great fault of omitting to mention numerous other sources of knowledge of equal or of greater value.

In several instances, moreover, erroneous assertions are made, or wrongful conclusions drawn. One or two of these we propose to discuss and correct

* In this respect our author has fallen far below the level attained by previous writers upon the subject. Compare for example: Uhlenhuth, *Handbuch der Photogen-und Paraffin-Fabrikation.* Quedlinburg Basse, 1858.

in this article. Our attention will be especially directed to the first chapter of Dr. Antisell's book—"History of the Art"—for in it are errors which have too long been current in the annals of chemical science—errors, the repetition of which by our author is the more unpardonable, since, from his very position, he should have known them to be such. Indeed, from statements to be found in various parts of his work, it would appear that he must have known of these errors—that he must have been in possession of most of the facts which will here be brought forward.

That we may form a correct notion of the subject under discussion, let us here digress for a moment.

As a general rule, when any bituminous substance is subjected to distillation—in the ordinary acceptation of the term, *i. e.*, when it is gradually heated in any appropriate apparatus, a quantity of an oily fluid is produced, which may be collected in receivers; small quantities of gas, water, and other incidental products being at the same time obtained.

The oily liquid, which alone interests us here, known in this country as *crude coal oil*, is a mixture of various hydrocarbons, among which the wax-like substance Paraffine is an almost never-failing constituent. Crude oil, though of course varying greatly, according to the sources from which it is derived, like the various marketable "coal oils" obtained from it by purification, is specially characterized by its low specific gravity, being capable of floating upon water.

When, on the other hand, a bituminous substance, instead of being gently and gradually heated, is suddenly exposed to the action of an intense heat—when, as in the ordinary process of gas-making, it is thrown into vessels of iron or clay, which have *previously* been brought to a bright red heat, a different set of products is obtained. A large quantity of permanent gas is produced, while the liquids formed are no longer the light oily compounds just spoken of, but are composed of another set of hydrocarbons which taken collectively, are *heavier than water.* These constitute coal-tar. Among them paraffine is no longer found, excepting in comparatively rare instances, another solid substance, Naphthaline, being a characteristic component of the mixture. When the process to which the bituminous matter is subjected is a mixed one, *i. e.*, when a portion of the substance comes in contact with strongly heated surfaces, while other portions receive only an amount of heat sufficient to distill off oils of the kind first described, a mixed product, containing both coal-oil and coal-tar, is naturally obtained. As an instance of such mixed product may be mentioned the tar obtained in the preparation of gas from Boghead coal,* it being almost impossible, in this case, to maintain the retorts at the temperature best suited for gas-making, on account of the great amount of heat which is rendered latent by the enormous volume of gas generated by this highly bituminous substance.

It should be mentioned, that both crude coal-oil and coal-tar contain a quantity of "light stuff," composed of several exceedingly volatile and inflammable liquids. Some of these naphtha-like fluids, for example benzol—the benzine of the French—(known as *benzule* in the private vocabulary of Dr. Antisell, or that of his proof-reader)—may occur both in crude-oil and in tar; others do not. We refer to these "light-stuffs" here merely for the purpose of explaining that they have been at times spoken of as "volatile oils," from the resemblance which they bear to spirits of turpentine and other essential oils, and to eliminate them from the discussion. They are of but minor interest at the present moment, when compared with the true "coal-oil" now so largely employed in this country. We may mention, in passing, that Dr. Antisell has very inconsiderately obscured his historical sketch of the progress of the art of distilling coal-oil

* In the same class are several Scotch cannels, our own Breckenridge and allied coals, also the Albert coal of New Brunswick and the like.

by blending with it the question of coal-tar naphthas. He has, for that matter, been unfortunate throughout in the presentation of this part of his subject; all the crude liquid products of distillation, at whatever temperature the process has been conducted, being indiscriminately classed by him as *tar*. Now, it is well known to practical men, as has already been described, that the products obtained from bituminous matters by slowly distilling them, is as different from coal-tar as ether is from alcohol. The term crude-oil, by which the first-named liquid is known to manufacturers in this country, characterizes it perfectly; so does the term *huile de schiste* (written at times simply "*schiste*") of the French.*

It is surprising that Dr. Antisell should have followed the example of several German authors—without their excuse—in thus perplexing his readers.

In returning from this digression, we would expressly declare our disbelief in the adage which allows for the existence of no novelty. Still we do believe that very few of the arts have sprung into existence in a day, their perfection, and especially their development, having almost always resulted from the successive labors of numerous individuals; and we do believe that the inventor, who first practically "applies" any abstract knowledge, and thus creates a new art or branch of industry, is entitled to credit therefore—and to far more credit, and that of a different order, than the man who subsequently introduces this art into a foreign country. We would not detract from the efforts of the latter; on the contrary, would accord them high praise; but we desire, first of all, to see justice meted out to him who created the art—to those who increase human knowledge, sooner than to its mere diffusers.

We would therefore join issue with Dr. Antisell when, in his preface, he tells us that his book is a "record of the origin and condition of an infant art," and again mentions "this new branch of industry." So, also, in the first lines of his Historical Introduction, where he speaks of "the new and extensive manufacture of oils from coal and other bituminous substances." For these statements are not only erroneous in themselves, but they—no less than the unfair allusions which appear on subsequent pages—tend to do great injustice to earlier inventors, and especially to the memory of a man whose name must ever remain inseparably connected with the history of the *art* of manufacturing the fluid now known as coal or paraffine-oil. We refer to SELLIGUE. More than twenty-five years ago, this inventor's method of obtaining such oil was described in the *Journal des Connaissances Usuelles*, for Dec., 1834, p. 285. (See also *Dingler's Polytechnisches Journal*, 1835, lvi, 40.) This article was subsequently followed by numerous others, until in Selligue's patent of March 19, 1845, we find the whole subject treated of most fully and clearly. As a lucid and truthful description of his processes and of the products obtained, this specification is most praiseworthy. Few subsequent writers upon the subject have been able to add anything to the stock of knowledge which it imparts. Taken for all in all, it is doubtless the most meritorious essay which has ever been published upon the art of manufacturing coal-oil. We can but reiterate our statement, that the brief, inaccurate, and exceedingly superficial comments which have been bestowed by Dr. A. (pp. 9, 80, etc.) upon the information which Selligue has imparted in his admirable series of essays, does great injustice to the subject as well as to this author.

Leaving for a moment the minute consideration of Selligue's improvements, let us first glance at the labors of some of his predecessors.

As Dr. Antisell has truly said (p. 7), the discovery of the production of oil from coal appears to date as far back as the time of Boyle, (1728-1790), when the well known experiments of Dr. Clayton were made.†

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* We may here observe, that throughout this article we shall translate the French term *huile de schiste*, by its English equivalent, *coal-oil*.

† Philosophical Transactions, Jan. 1739, No. 452, p. 59; in Martyn's Abridgment, vol. ix. p. 395.

In distilling coal from a pit near Wigan in Lancashire, this observer obtained, first phlegm (water), then oil, and finally gas.

No doubt an earlier record of similar experiments might be found in the writings of the alchemists, who, as is well known, subjected almost every substance to processes of distillation.

During the last century attention was again several times called to the fact.*

It would seem, however, that nothing very definite was published before the year 1830. UNVERDORBEN† had, indeed, in the preceding year, called attention to oils distilled from petroleum, and even appears to have obtained paraffine—to which however he gave no name.‡ The attention of the scientific world was first really attracted to this substance by the memorable memoir of Reichenbach,§ who separated it, in the first instance, from wood tar, and described its properties at length. In the following year, Reichenbach‖ is at great pains to prove that the crude-oil, obtained by slowly distilling coal, contains no naphthaline,¶ that naphthaline is not a product of the slow distillation of coal, but is a result of the subsequent decomposition of such products by heat; and that the coal-tar of gas-works is not crude-oil, but an impure mixture of the products of distillation with those resulting from their decomposition.**

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* In addition to the authorities cited by Dr. A. (p. 8), we would mention the following from *An Experimental History of the Materia Medica, or of the Natural and Artificial Substances made use of in Medicine;* by WILLIAM LEWIS, M.B., F.R.S., 3rd Edit. 8vo, Dublin, MDCCLXIX, vol. ii, p. 143, Article *Petroleum;* also, (according to American Druggists' Circular, iv, 36,) in the London edition of Lewis. 4to, 1761, p. 436:

"Some mineral oils, procurable among ourselves, are used by the common people, and often with benefit. The empirical medicine, called British oil, is of the same nature with the petrolea; the genuine sort being extracted by distillation from a hard bitumen, or a kind of stone coal, found in Shropshire and other parts of England."

† *Berzelius's Jahresbericht,* x, 181, from *Kastner's Archiv,* xvi, 122; also in *Schweigger-Seidel's Journal für Chemie und Physik,* 1829, lviii, 243.

‡ For allusions to other earlier German researches bearing upon the subject, see Reichenbach's Memoirs, which will be cited directly. Compare also Gmelin's Handbook of Chemistry (Cavendish Soc. Edit.), xii, 439.

§ *Journal für Chemie und Physik,* (or *Jahrbuch der Chemie u. Physik, Band,* xxix) *von* Schweigger-Seidel, 1830, lix, 436.

‖ *Ibid,* (or *Neues Jahrbuch der Chemie u. Physik,* B. 1,) lxi, 175.

¶ Dr. Antisell dismisses this article (p. 11) with the statement that "in 1830-'31, Reichenbach discovered naphthalin." It may not be amiss to state that naphthaline was discovered at least ten years earlier, having been described by GARDEN in 1820 (Thomson's *Annals of Philosophy,* xv, 74), to whose labors as well as to those of CHAMBERLAIN, KIDD, and others, Reichenbach particularly refers in this very article. See also *loc. cit.* B. lxviii, [B. viii, of the "*Neues-Jahrbuch,*"] S. 233.

** It must here be explained that Reichenbach has suffered great injustice at the hands of those who, in translating portions of his papers, have rendered his term "*Steinkohlentheer*" literally—coal-tar. Now the term coal-tar, in countries abounding in gas-works like England or the United States, means the tar of gas-works, and it means nothing else. Gas-works, it must be remembered, were, until quite recently, by no means so common in Germany, and were doubtless rare enough in 1830, consequently, it is not at all strange that the English idea of "coal-tar" should not have become current in that country. Reichenbach, for that matter, distinctly and repeatedly asserts, that his "*Steinkohlentheer*" is a very different substance from the tar of gas-works. In a word, it was *crude-oil.* If, perchance, there may be any person who would accuse us of mistranslating certain *words* used by Reichenbach, we would at once refer such an one to the orginal memoirs of this author. Submitting it to the judgment of any competent chemist, whether we have misinterpreted his lan-

These experiments were made upon a manufacturing scale, Reichenbach being, at this time, "chief of an extensive system of mines, iron furnaces, machine shops, chemical works, etc., most of them established by himself on the estate of Count Salm [Blankso, Moravia]. These works lie along a line some fifteen miles [5 *Stunden*] in length." (Schweigger Seidel).

In another article published later, in 1831,* he describes his method of obtaining paraffine from the distillation of flesh and of *coal* (portions of 75 lbs. weight having been operated upon). With regard to coal, he particularly urges the necessity of slow distillation, in order to prevent the decomposition of the first products and the consequent formation of naphthaline, as explained in his previous article, to which he refers. The paraffine was separated from the less volatile portions of the rectified oil by cooling—the description of which oil R. reserves for a separate article.† He also obtained paraffine from petroleum. Two more papers upon the subject were published by Reichenbach in this year,‡ only the first of which is of particular importance in this connection. It relates to *Eupion* (εὖ very, πίον fat). A term by which Reichenbach designates, in some instances, a portion, in others the whole of the somewhat difficultly volatile, fat-like oils, prepared by purifying the first product obtained by slowly distilling substances of animal or vegetable origin. This eupion was, in fact, a mixture of several hydrocarbons—the same which, in similar mixtures, are now collectively known in commerce as coal-oil; called paraffine oil by some, and designated in the retail trade by innumerable other names of only local significance.

Eupion was obtained by Reichenbach from the products of the slow distillation of animal and vegetable substances, as well as from coal, and was minutely described by him. We make but a single extract from this article, which occupies some thirty-two pages: "When any one shall succeed in separating eupion, at a sufficiently cheap rate, from the tars [crude-oils], it will very probably enter into the circle of substances useful in household economy. For, since it burns from a wick, brightly and clearly, and is free from smoke, it is in no wise inferior to the finest oil as an illuminating material. It does not grease nor crust the wick, nor stiffen when cold. If we consider, in addition to this, that for all purposes where cold can exert no influence, the paraffine need not be separated, but can be left dissolved in the eupion, and used in conjunction with it for lighting; we shall perceive that this is of some importance, since the two substances are thus mutually improved for technical purposes."

In 1832, Reichenbach§ again published a note upon eupion; and, in 1834, another long article,‖ in which he once more dwells upon its useful properties.

Reichenbach's contributions on the subject of the dry distillation of organic substances, are comprised in some twenty or more long articles, not counting

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guage, [compare, for example, *loc. cit.*, B. lxviii., [B. viii, of the *Neues-Jahrbuch.*,] S. 226].

It may be worth while also to call the attention of the reader to the fact that all of the substances discovered by Reichenbach in "tar" (as the text-books tell us) were in reality obtained from crude-oil. Knowing this, every one familiar with recent chemical literature, will perceive at once why so few of R.'s scientific results have been corroborated. For, until quite recently, the attention of chemists interested in such researches, has been almost completely occupied with the subject of coal-tar. Compare also Reichenbach's complaint against Dumas and Laurent, in Schweigger-Seidel's *Journal für Ch. u. Phys.*, 1838, lxviii, 223.

* *Loc. cit.*, lxi, [or B. 1, of the *Neues-Jahrbuch*], S. 273.

† *Vid. infra.*

‡ *Loc. cit.*, lxii, [or B. ii. of the *Neues-Jahrbuch*], S. 129, 273.

§ *Loc. cit.*, B. lxvi, [B. vi. of the *Neues-Jahrbuch*], S. 318.

‖ Erdmann's *Journal für praktische Chemie*, i, 377.

several smaller "notes." A tolerably complete list of which may be found in Erdmann's Journal für praktische Chemie, i, 1. It is very much to be regretted that these memoirs have never been collected and published as a separate volume. Even now, any chemist who could find time to collect these scattered articles and translate them into English or French, would unquestionably promote the cause of science by so doing.

Looking at the question for a moment, solely in its scientific bearings, we cannot refrain from an expression of astonishment, that the details of Reichenbach's researches are so little known to the generality of chemists;* while, on the other hand, we are forced to confess, that it is indeed rare that scientific researches, conducted by a chemist in his laboratory, have so fully described a future art—have so accurately pointed out the methods to be followed and precautions to be observed by the practical manufacturer. We must not omit to mention that, in 1831, Christison† of Edinburgh made known his discovery of paraffine in petroleum from Rangoon. Not knowing of Reichenbach's previous publication, Christison named it *Petroline*, but subsequently admitted its identity with paraffine. In 1833, Bley‡ distilled oils from lignite.

A little later, in 1834, Gregory§ published an able article upon paraffine and eupion, and their occurrence in petroleum. Of this memoir we cite but two lines, (vid. Trans., p. 129, or Rep., p. 113). "It follows," says Gregory, "that there are some kinds of naphtha [petroleum] which contain paraffine and eupion, and are consequently the results of destructive distillation."

In the following year, v. Kobell‖ also noticed paraffine in petroleum.

For the labors of Hess in Russia, and of several other chemists in Germany, as well as for the interesting discussions which followed between these

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* This lack of information appears to depend upon the circumstance, that the writers of most recent chemical text-books seem to have derived their knowledge of the subject in question, from Gay Lussac's brief abstract of Reichenbach's earlier memoirs, which was published in 1832, in Poggendorff's *Annalen*, xxiv, 173; also in the *Annales de Chimie et de Physique*, [2], 1, 69; and quite extensively copied by the journals of the day.

In this connection we would respectfully urge upon all those who have fallen into the common habit of regarding as somewhat apochryphal the numerous substances of greater or less scientific interest, which Reichenbach separated from the products of dry distillation, that before seeking to discredit—or allowing themselves to disbelieve—them, they should conduct experiments similar to his, *on a scale of equal magnitude.* Let us here also bear in mind the luminous conclusion of the late Dr. Hope of Edinburgh, who, as the story goes, (Vid. London Chemical News, i, 56), one day informed his class that Reichenbach had discovered in tar, "creosote, picamar, paraffine, cedriret, capnomor, and a host of other substances of no interest or importance whatever." Of these "unimportant" substances, two at least, eupion and paraffine, are to-day as well known, in the world, as bees-wax or spermaceti, although comparatively little—we had almost said nothing—has been added to the scientific knowledge of them, since the publication of Reichenbach's memoirs. If, perchance, any other of these well-nigh forgotten bodies should be found to possess any technical importance, we would quickly enough find some one claiming credit for its "discovery," and oppressing chemical nomenclature, by adding yet another name to the existing "host." Even now we await, with no little interest, the elucidation of the question—whether the new violet dye, prepared by oxydizing anilin, which is exciting so much interest, under the names anilein, Perkin's purple, *mauve*, etc., is not identical with, or a component of, the *pittical* of Reichenbach.

† Transactions of Royal Society of Edinburgh, xiii, 118; also in Repertory of Patent Inventions, 1835, [N. S.] vol. iii, p. 390.

‡ Schweigger-Seidel's *Journal für Chemie u. Physik*, B. lxix, [B. ix, of the *Neues Jahrbuch*], S. 129.

§ *Transactions of Royal Society of Edinburgh*, xiii, 124; also in *Repertory of Patent Inventions* 1835, [N. S.] vol. iv, p. 109.

‖ J. pr. Chem. v. 213.

observers and Reichenbach, the reader may consult the general index [Namen-u. Sach-Register zu den Bänden i. bis lx, Leipzig, 1845] to Poggendorff's Annalen der Physik u. Chemie.

At the same time that these scientific researches were in progress in Germany and Scotland, or even earlier, numerous practical efforts to manufacture oils from bituminous substances were made in France.

Although the precise date at which these experiments were commenced is somewhat obscure, it will not be difficult to trace the history of the successful development of the industry to which they gave rise.

As stated by Dr. Antisell, the MM. Chervan* had a patent, dated in 1824, for distilling bituminous substances. Blum and Moneuse,† in 1832, claim only the application of coal-oil to purposes of lighting—not its manufacture, which they allude to as being well known.

Subsequently (7th October, 1833) Boscary‡ obtained a patent for extracting pyrogenous oil from different substances, asphaltums, etc., and especially from the shales which occur in the environs of Autun (*Saone* et *Loire*), and finally from all the bituminous matters in France. The oil, which is obtained by distilling the shale in metallic cylinders, may be used, according to Boscary, instead of fish-oil or resin, for gas-making—a much better gas than that prepared from coal being thus obtained.

In 1833, Laurent§ occupied himself with the investigation of various bituminous shales, both French and English, at the instance of the MM. Blum, whom he mentions as being occupied with the distillation of oil from the shales of the environs of Autun.‖ Laurent gives the details of the process employed by himself, telling us that the retort in which his shales were distilled attained a sombre red heat at the close of the operation; also of the percentage amounts of oil (20 p.c.), gas, coke and water obtained from the Autun shale; how the oil cannot be burned in ordinary lamps, on account of smoking, but affords a very luminous flame when consumed in lamps furnished with suitable chimneys. He shows moreover that the oil contains paraffine, and does not contain naphthaline.

Laurent subsequently published another paper¶ upon this oil, in which article he records his efforts to ascertain what definite chemical compounds are contained in the oil. One of the products obtained by fractional distillation, viz., an oil boiling at 167° to 170° (C.) = 333° to 338° F., he considers as identical with eupion.

In 1834, we find, for the first time, an article** describing the process of Selligue, although it would appear from the statements of this chemist and of others, that his attention had been directed to the subject of distilling bituminous shales several years earlier. The cited article relates how the shale is *slowly* distilled in iron cylinders, until no more oil comes over; how the oil obtained is characterized by containing neither oxygen nor naphthaline, but a solid substance differing from the latter, and resembling that called paranaphthaline†† by Laurent.

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* Brevets d'Invention xviii, 232. † *Ibid.* lxv. 250.

‡ *Ibid.* lxviii, 359.

§ *Annales de Chimie et de Physique*, liv, 392.

‖ According to Laurent, he had himself proposed to a company, in 1829, to work these shales, in order to extract the oil contained in them, and to employ it for lighting.

¶ *Comptes Rendus*, 1837, iv, 909; more fully in *Annales de Chimie et de Physique*, lxiv, 321.

** *Journal des Connaissances Usuelles*, Dec. 1834, p. 285; also in Dingler's *Polytechnisches Journal*, 1835, lvi, 40, from which our extract is taken.

†† The inadvertency of confounding this body with paraffine was subsequently corrected by Selligue.

In 1834, '35 and '36, Selligue* was principally occupied with his well-known process for making water-gas. In calling the attention of the French Academy† to this, he remarks that, in conjunction with David Blum, he holds a patent granted in 1832 for the application of oils obtained from shale to purposes of direct illumination, and that the working of the shale employed is in the hands of a company capable of developing the business to any extent which commerce or the arts may require.

In the same year Payen,‡ in reporting upon Selligue's water-gas, remarks upon the great importance of the new industry of distilling oil from shales which S. has introduced.

In the following year we again find Selligue before the Academy§ requesting that body to appoint a committee to examine the merits of his new system of gas-lighting; his process of distilling bituminous shales on the great scale by means of apparatus, each one of which furnishes from 1,000 to 1,400 pounds of crude oil per day—this being about 10 per cent of the weight of the shale employed, and being almost all which exists in the raw material; also of his process of separating various products from the crude oil, some of which are applicable to the production of gas, others to ordinary purposes of illumination, and others to different uses in the arts. This petition was referred to a committee of three—Thenard, D'Arcet and Dumas—who reported in 1840.‖ They mention the localities of Selligue's three establishments for obtaining oil from shales; the amounts of oil obtained from the different kinds of shale, &c.

In 1838 Selligue also obtained a new patent¶ "for the employment of mineral oils for lighting." In his specification he informs us that the principles upon which his processes for rendering the oil obtained from shales proper for the purposes of direct** illumination depend, are:

I. Removal of almost all odor. II. Removal of all tar. III. Removal of the most volatile portions of the oil, which are also the most inflammable and the most odorous, the presence of which would cause the oil to have too great fluidity for the capillarity of ordinary wicks. * * *

The operations, continues Selligue, consist in slowly distilling the bituminous shale, and collecting the liquid products in large receivers. These products are redistilled, and divided into fractions by refrigerating. They are treated with concentrated sulphuric acid for a longer or shorter time according to the nature of the shale employed. Twenty-four hours are ordinarily sufficient, the oil being agitated from time to time. The quantity of acid used varies from $\frac{1}{10}$ to $\frac{1}{20}$. After this the oil is to be carefully drawn off from the tar, and washed with water. Slaked lime is then added and a current of steam passed through the oil in order to carry off by distillation all the more volatile and odorous liquids. This last, says Selligue, is the most important part of my process, for if this very inflammable portion were allowed to remain in the oil, one could not use the latter in ordinary lamps *à courant d'air.* * * * This patent it should be observed claims only to be an improvement upon that of Blum and Moneuse (*vid. Supra*). Selligue asserts, however, that coal-oil had never before been prepared in such a manner that

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* See seven patents in *Brevets d'Invention*, lxx, 269. Of these patents two are dated 1834; two, 1835; and three, 1836. For a description of his process of gas-making see also *Bulletin de la Société d'Encouragement*, Oct. 1838, p. 396; or Dingler's *Polytechnisches Journal*, lxxi, 29.

† *Comptes Rendus*, 1837, iv, 969.

‡ Dingler's *Polytechnisches Journal*, lxviii, 201; from *Bulletin de la Société d'Encouragement*, Dec. 1837, p. 493.

§ *Comptes Rendus*, 1838, vii, 897.

‖ *Comptes Rendus*, x, 861; also in Dingler's *Polytechnisches Journal*, lxxvii, 137.

¶ *Brevets d'Invention*, lxviii, 395.

** The term "direct illumination" is constantly used by Selligue in contradistinction to the indirect use of the oil in his process of gas-making.

it was fit for use in common lamps. This has, indeed, he says, been the subject of many researches, but no one has hitherto succeeded in avoiding the empyreumatic odor, and the very inflammable products which caused the oil to rise too quickly to the summit of the wick. He goes on to define the difference between his purified oil and the crude oil obtained directly from shale. On the 27th of March, 1839, Selligue specifies certain additions and improvements to the preceding patent. I should add, he says, that I now divide the products of distillation into four distinct parts, which afford me every facility for employing these products in the arts and for domestic economy. In these divisions there are indeed some anomalies which arise from differences in the shales, &c. which I treat; but the following products are always obtained:

I. A light, volatile oil more or less odorous according to the source from which it is derived. * * * This can be used in painting, for dissolving resins, &c., for lighting by vaporising it (it being very volatile) or for the production of gas according to my system.

II. A fat oil only slightly volatile, and having but little odor; this can be used for domestic purposes in ordinary lamps with or without admixture of animal or vegetable oils.

III. A fatty substance almost odorless, possessing all the properties of the fats, and well adapted for use in the arts. It can also be used for lighting, either by mixing it with light oils or by decomposing it for the production of gas. It can moreover be used for soap* since it saponifies very well, and being without odor affords a very good soap; with ammonia the fat forms a sort of pomade.

IV. An odorless pitch of great purity and tenacity, suitable for preparing a black solid varnish for preserving wood, iron-work, &c. * * *

In 1839, Selligue† in alluding to the use of his oils in the treatment of cutaneous diseases speaks of the three large establishments for the distillation of bituminous shale which he has erected in the Department Saone et Loire, and mentions the fact that the oil (crude ?) is furnished at the rate of about two cents [ten centimes] per pound.

The question of price is again discussed a few years later, when Selligue‡ says: it has been stated that crude shale oil costs only $1 50 per 100 pounds, and that it contains 60 per cent of a very light volatile ethereal oil well suited to afford light, as well as 40 per cent of a fat substance. Now since 1837, I have extracted more than 4,000,000 pounds of oil from bituminous shale, but the oil (crude ?) costs 20 cents a gallon (22 frs. the hectolitre) or even 27 cents when delivered in Paris. From every hundred measures of the crude oil are obtained (by distillation) 20 measures of volatile oil boiling at 100° C.=212° F.; 30 measures of less volatile oil boiling at 150° to 260° C.=302° to 500° F.; 14 measures of an oil containing paraffine, and 28 measures of fat—five measures being lost. In purifying these products a further portion is lost.

The clearest of all Selligue's specifications, however, is that of the patent granted to him March 19, 1845§ for the distillation of bituminous shales and sandstones.

After describing the various forms of apparatus used in distilling, into one of which superheated steam was introduced, he enumerates the products of distillation as follows: I. A white, almost odorless, very limpid mineral oil—somewhat soluble in alcohol—which may be used as a solvent, or for purposes of illumination in suitable lamps, &c.

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* This "soap," (emulsion) is described more fully in the sequel.

† *Comptes Rendus*, ix, 140; also *Annalen der Pharmacie*, von Wœhler u. Liebig, xxxii, 123.

‡ Dingler's *Polytechnisches Journal*, xci, 193; from the *Moniteur Industriel*, 1843, No. 770.

§ *Brevets d'Invention*, [new series, (loi, du 5 Juillet, 1844,)] iv, 30.

II. A sparingly volatile mineral oil of sp. gr. 0·84 to 0·87, of a light lemon color, perfectly limpid, almost odorless, never becoming rancid, and susceptible of being burned in ordinary oil lamps, of constant level *à reservoir superieur*, with double current of air—a slight modification of the form of the chimney and burner being alone necessary. This oil can also be mixed with the animal or vegetable oils. Oils thus prepared do not readily become rancid, nor do they congeal easily when subjected to cold.

III. A fat mineral oil, liquid at the same temperature as olive oil. This oil contains a little paraffine; it is peculiarly well adapted for lubricating machinery, and has an advantage over olive and other vegetable oils, or neatsfoot oil in that it preserves its unctuosity when in contact with metals and does not dry up. It saponifies easily, and forms several compounds with ammonia.

IV. From the oils Nos, I, II, and III, I extract a red coloring matter which can be used in various arts.

V. White crystalline paraffine which needs but little treatment in order to be fit for making candles; this substance does not occur in very large proportion in the crude oil, and the proportion varies according to the different mineral substances upon which I operate. There is but little of it in petroleum, and in the oil obtained from bituminous limestone. I often leave a great part of the paraffine in the fat oil and in the grease in order that these may be of superior quality.

VI. Grease. This grease is superior to that of animals for lubricating machinery, and for many other purposes, since it does not become rancid, and remains unctuous when in contact with metals.

VII. Perfectly black pitch—very "drying"—suitable for preserving wood, metals, &c.

VIII. An alkaline soap obtained by treating the oils with alkalies.

IX. Sulphate of ammonia. X. Manure prepared by mixing the ammoniacal liquor, or the blood of animals, with the crushed fixed residue (coke) of the shale. XI. Sulphate of alumina from the residue of the shale. F. H. S.

[*To be concluded.*]

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On Chlorophyll.*—Frémy has communicated the results of an interesting investigation of the green coloring matter of plants. His conclusions are as follows:

1. Chlorophyll may be decomposed into a blue and into a yellow substance.

2. These substances give compounds with alumina which are insoluble in water. When hydrate of alumina is added to an alcoholic solution of chlorophyll, neither of the constituents of the latter unites with the earth. When, however, water is added gradually, the blue substance and a little of the yellow first unite with the alumina to form a dark green precipitate, while the alcoholic solution remains yellow. Both bodies are precipitated together from a very dilute aqueous solution.

To separate the blue and yellow substances completely, the author introduces a mixture of two parts of ether, and one part of muriatic acid, diluted with a little water into a bottle, and shakes the whole till the mu-

riatic acid is saturated with ether. The natural chlorophyll which may be extracted from leaves by means of alcohol, may be easily decomposed into its constituents by shaking it with this mixture of muriatic acid and ether. The liquid first takes a brown color, after which the aqueous muriatic acid takes a beautiful blue, while the ether separates, holding the yellow coloring matter in solution. Frémy calls the blue substance phyllocyanin, and the yellow phylloxanthin. When alcohol is added, so that the two liquids mix, a green is again produced, of the color of the natural chlorophyll.

Young leaves exposed to the vapors of muriatic acid, quickly assume a beautiful green color. The yellow autumn leaves, on the other hand, contain only phylloxanthin. Under certain circumstances, phyllocyanin is capable of yielding another yellow substance, to which the author gives the name of phylloxanthein; from this substance phyllocyanin may be again obtained. The blue coloring matter of chlorophyll is more easily altered than the other. The author promises a further investigation of this very interesting subject; his results are of equal interest to naturalists and chemists.—*Compt. Rendus*, l, p. 405.

2. *On the Separation and Estimation of Phosphoric Acid.*—Chancel has given a method of estimating phosphoric acid, which depends upon the entire insolubility of phosphate of bismuth in liquids containing free nitric acid. When a solution of acid nitrate of bismuth, so dilute as not to be rendered turbid by water, is added to a liquid containing a phosphate dissolved in nitric acid, a very dense, white precipitate is immediately formed, which has the formula BiO_3,PO_5, and the constitution of which, according to Chancel, is perfectly constant.

The neutral phosphate of bismuth is perfectly insoluble in water, and dilute nitric acid, whether cold or hot; it dissolves sensibly in solutions containing a large proportion of salts of ammonia. The filtration and washing is very easy and rapid; the dried salt may be ignited in a platinum crucible, and does not fuse at a red heat.

Pyrophosphoric acid is also precipitated completely by the acid nitrate of bismuth; a white precipitate is formed which is much more voluminous than the tribasic phosphate. This precipitate has the formula $2BiO_3.3pPO_5$: it is easily and completely transformed into the tribasic phosphate by simply boiling it in the presence of an excess of the acid nitrate of bismuth.

The metaphosphates behave in the same manner, but the precipitate requires a longer boiling to be completely transformed into the ordinary phosphate.

Chancel asserts that he has been able to detect the presence of one milligram of phosphoric acid when mixed with 120mm. of alumina in a dilute solution containing more than a gramme of free nitric acid. As the precipitation is very rapid in a hot solution, and as the liquid becomes almost instantly clear, it would be easy to estimate phosphoric acid by a titrated solution of acid nitrate of bismuth.

Chancel prepares the acid nitrate by dissolving, with the aid of heat, one part of pure crystalline subnitrate of bismuth in four parts of nitric acid of specific gravity 1·36, adding to the solution 30 parts of distilled water, and filtering if necessary. In using this solution, the phosphate,

if not soluble in water, is to be dissolved in nitric acid, avoiding a large excess. The solution is to be diluted with water, the nitrate of bismuth added as long as a precipitate is produced, and the whole brought to boil, filtered and washed with boiling water. After drying, the filter must be separated as completely as possible from the precipitate, burned by itself, and the ashes added to the precipitate, which may be heated to redness in a platinum crucible. The bases are easily estimated in the filtrate, after removing the excess of bismuth by sulphuretted hydrogen. This process requires that the liquid should be free from chlorids and sulphates, which, when present, are easily removed by nitrate of silver and chlorid of barium.—*Comptes Rendus*, l, p. 416.

3. *On a New Mode of Preparing Calcium.*—CARON has succeeded in preparing large quantities of calcium by the following process: A mixture of 300 parts of fused and pulverized chlorid of calcium with 400 parts of granulated distilled zinc and 100 parts of sodium in pieces is to be heated to redness in a crucible. The reaction is feeble, and after some time flames of zinc appear. The heat is then to be moderated, the temperature remaining as high as possible without volatilizing the zinc; after a quarter of an hour the crucible may be withdrawn from the fire. It contains a well fused metallic mass, which is highly crystalline, and which contains from 10 to 15 per cent. of calcium. The alloy is then to be placed in a crucible of gas-retort carbon and the zinc expelled by heat: in this manner Caron obtained masses of 40 grammes at a single operation, and containing only the impurities of the zinc employed. As thus obtained calcium has a brass-yellow color and a density of from 1·6 to 1·8, it is not sensibly volatile, but filings of the metal burn with red sparks of remarkable beauty without formation of vapor, which seems to show that the metal is not volatile at the temperature of its combustion. The author promises to communicate the results of similar experiments in the preparation of barium, strontium, &c.—*Compt. Rendus*, l, p. 547.

4. *On a New Metallic Element.*—VON KOBELL has discovered in euxenite, æschynite, and samarskite, and a tantalite from Tammela, a new metallic acid belonging to the same group with tantalic and niobic acids. To the new metal contained in this acid, the author has given the (not very well selected) name of *Dianium.* When dianic acid, as precipitated by ammonia from its solution in chlorhydric acid, is boiled with chlorhydric acid and metallic tin, a beautiful deep sapphire blue solution is produced, which remains blue after filtration. When tantalic acid, from the tantalite of Kimeto, or niobic acid from Bodenmais, are treated in the same way, the solution becomes bluish; and on adding water, the color quickly vanishes, and the solution, on filtering, passes through colorless.

When dianic acid is boiled with chlorhydric acid and zinc, instead of with tin, the blue solution does not appear, the precipitated acid becomes blue, but filters colorless, and is decolorized by water without being sensibly dissolved. When equal quantities of dianic, tantalic, and hyponiobic acid are boiled with concentrated chlorhydric acid, upon a funnel of platinum-foil for three minutes, all three give yellowish milky liquids; if water be then added, the solution of dianic acid becomes perfectly clear, while the tantalic and hyponiobic acids remain undissolved.

When freshly precipitated dianic acid is heated to boiling with dilute sulphuric acid, the milky liquid poured into a glass, and grains of distilled zinc thrown in, the dianic acid in a few moments becomes smalt blue, even dark blue, and retains this color for some time on addition of water; but the liquid passes through the filter colorless. In this respect, dianic acid resembles hyponiobic acid, while tantalic acid, under the same circumstances, becomes pale blue, and immediately loses this color on addition of water. In this manner, tantalic may be distinguished from dianic and hyponiobic acids. The relations of the three acids to chlorhydric acid and tin, and to sulphuric acid and zinc, are thus sufficient to distinguish them from each other.

Dianic acid appears to exist, though in a less pure state, in the tantalite from Greenland, in pyrochlore from the Ilmengebirg, and in the brown Wöhlerite—though the author had but small quantities of these minerals at his disposal. A small piece of black yttrotantalite, believed to be from Ytterby, gave the reaction of dianic acid. A second specimen, however, the specific gravity of which was found to be 5·55, contained tantalic acid.

Titanic acid is easily distinguished from the other acids of the same group, by boiling it with muriatic acid and tin, and diluting the solution with water. The blue color then passes to rose red, and the solution retains this color several days. When dianic acid is present, the blue color predominates, but after standing some hours the rose color of titanic acid appears.

The tantalite from Tammela, which Von Kobell terms dianite, has a specific gravity of 5·5—while the other tantalites vary in density from 7·06 to 7·5. The streak of dianite is dark grey, while that of the tantalites from Tammela is dark brown red. Before the blowpipe, dianite exhibits no sensible difference from the tantalite of Kimeto.

H. Rose, to whom Von Kobell sent a specimen of dianic acid for examination, considered it probable that the peculiar reactions of this substance might be due to the presence of tungstic acid. Von Kobell has, however, shown by special experiments, that this is not the case. In conclusion, the author recommends those who desire to repeat his experiments, to employ the same proportions of water, acid, etc., of which he himself made use, and for the details of which we must refer to the original paper.—*Bull. der Acad. der Wissenschaften*, March 10th, 1860, (Munich). W. G.

5. *Note on the extent to which Mercury volatilizes along with the vapor of water at* 100° C.; by J. W. MALLET.*—In Berzelius' Traité de Chimie it is stated that Stromeyer drew attention to the fact of the evaporation of mercury in considerable quantity at 60° to 80° C. with the vapor of water, the more volatile substance carrying with it the less volatile, as in the case of a solution of boracic acid when heated.

This fact does not seem to have been very generally noticed by the compilers of chemical text-books in treating of the history of mercury, though it is always stated that the metal is capable of volatilizing *to a very slight extent*, even when alone, at the common temperature of the atmosphere. Some doubt too would seem to have been thrown upon Stromeyer's observation by the experiments made, under peculiar condi-

* Communicated by the author.

tions, by Fresenius, and reported by him in the appendix to his treatise on quantitative analysis. It was found that 6·4402 grm. of mercury, *covered* with a considerable quantity of water, and heated to the boiling point of the latter for a quarter of an hour, lost but ·0004 grm., while exposure to the air at summer heat for six days produced a further loss of ·0005 grm.

I have lately observed the tolerably rapid evaporation of the metal with steam, produced not from a mass of water covering the quicksilver, but from a porous and highly absorptive clay which was dry to the touch, but gave off some eight or ten per cent of water when heated to 100° C. A specimen of this soil had been in contact with mercury, and several grams of the metal in small globules had become mixed up with the mass. It was placed in the common copper box with double sides which serves as a steam-bath, and exposed to the temperature of boiling water. In half an hour I was surprized to see that the little piece of glass tube through which the heated air and vapor from the inside of the steam-bath escapes was coated with a bright specular deposit of metallic mercury. This was brushed off, and the tube replaced, when again it became in a short time similarly coated. The steam-bath was kept in constant use in drying this and other specimens of soil for twenty or thirty hours, and in this time between four and five grains of mercury was collected from the glass tube. Doubtless the condensation of the mercury vapor on the latter must have been far from complete—a large proportion of mercury probably escaped into the atmosphere. At any rate it seems clear that the metal can volatilize in very considerable amount when surrounded by vapor of water at 100° C., and not at the same time pressed upon or affected by the cohesion of a mass of liquid water. Hence an obvious necessity for thoroughly effective condensation when mercury is to be determined in its compounds by ignition with soda-lime.

TECHNICAL CHEMISTRY.

6. *Disinfectants.*—The use of a mixture of coal-tar and plaster-of-Paris for purposes of disinfection and for dressing wounds, as proposed by CORNE and DEMEAUX (*Comptes Rendus*, xlix, 127; see this Journal, xxviii, 425), has been recently reported upon in the French Academy by a committee—Chevreul, J. Cloquet, and Velpeau (*rapporteur*)—to which the subject was referred in July, 1859.

The great interest, which this method,—so favorably commented upon by the distinguished surgeon Velpeau soon after its publication,—excited among the medical men of France gave rise to the publication of numerous other systems of disinfection, which being submitted to the Academy for its approval were also referred to the committee in question. The labors of its members have thus been materially increased, and their report swelled to the dimensions of a general treatise upon disinfectants—especially those applicable to wounds.

In numerous experiments made at the Hospital *de la Charité* the mixed coal-tar and plaster of Corne was exployed, both in the state of powder and as a poultice made by mixing it with oil. When applied as a thick layer, three or four times a day, upon putrid, gangrenous and sanious wounds, the powder destroyed their odor without giving rise to any special pain. Upon indolent sores, however, or upon recent burns,

the contact of the powder produced considerable smarting upon some patients, though well borne by others. Wounds of the first class were often found to be cleaned as well as disinfected; while those of the second class generally acquired a dirty, pale, gray tint, their circatrization being hindered.

The poultices were found to be more advantageous than the powder in the treatment of cavernous wounds, purulent or fetid, and sinuous foci, open suppurating abscesses, anthracoidal suppurations, etc.

Applied directly to the sore, the poultices destroyed the putrid odors, allayed the inflammation without augmenting the pain, leaving beneath them a healthier pus, and the surfaces in better condition. In a word, the mixed coal-tar and plaster, when properly applied, disinfects wounds and putrid suppurations. As for the absorbent and detergent qualities which its inventors also claim for it, these are less clearly evident.

The powder absorbs better than the poultices,—the latter, it is true, take up a portion of the morbid exudations, but unless the dressing is carefully renewed, five or six times a day, pus will nevertheless collect beneath it. From this it follows that after having been somewhat cleansed the wound ceases, at the end of a few days, to clean itself, or to heal more rapidly than it would with the usual topical applications.

Upon ulcerated cancers, the mixture, either as powder or poultice, disinfects them partially, but neither dries up the suppuration nor alleviates the pain.

It is in the dissecting-room, upon organic matter in a state of putrefaction, that the mixed coal-tar and plaster is all powerful. The most infectious masses, when imbued with the powder, or simply rolled about in it, lose at once their disagreeable odor. According to Velpeau, his autopsy room was as approachable towards the close of last summer as it had formally been repulsive. It was freed from flies and other insects, as well as from putrid odors.

Although it would have been out of the province of the committee to experiment upon the application of this mixture in disinfecting filth upon the great scale, they have nevertheless proved that it can be advantageously used in hospitals for deodorizing urine or fecal matters.

The following inconveniences, to which the use of the mixture in surgery would give rise, are enumerated:

It not only soils the clothes of the patient, but hardens them and causes them to weigh more heavily upon or about the wound; it imparts to the bandages, with which the poultices are covered, a very tenacious rusty or yellow color; it must be frequently renewed, and although it destroys putrid smells, it retains a bituminous odor by no means agreeable to many persons.

These inconveniences are of comparatively slight importance, it is true, and may possibly admit of being remedied.

Of the other disinfectants submitted to the committee, several were only modifications of that of Corne and Demeaux:—Vegetable tar, as shown by Renault, may be substituted for coal-tar.—A mixture composed of hydraulic lime and tar did not disinfect wounds to which it was applied, nor could it be supported by the patients. With regard to the assertions of some practitioners, that common earth, talc, flour, or other

vegetable and mineral powders—even poudrette—when mixed with coal-tar furnish a more convenient and less costly disinfectant than that prepared with plaster, the experiments of the committee have proved that while coal-tar, mixed with common earth, well dried, or with sand, is equally, or perhaps much more, efficacious for disinfecting fecal matter as when mixed with plaster; that while comparative experiments made from this point of view upon sulphate of lime, clay, charcoal, linseed meal, and earth have resulted in favor of the latter, the same is by no means true in surgery. When applied to wounds or infectious suppurations these different mixtures were only partially successful, having proved to be less efficacious than the mixed plaster and coal-tar. In like manner the proposal to use an emulsion of coal-tar and tincture of saponine has not been found advantageous in practice: most patients complained of it, their wounds exhibited scarcely anything satisfactory, while the disinfection was very imperfect. The mixture of plaster and coal-tar was substituted for it, upon the same wounds, with decided advantage.

Although the modifications of Corne and Demeaux' process have not been particularly felicitous thus far, they have nevertheless served to confirm the fact that in reality it is the coal-tar which acts the principal part as disinfectant in these various mixtures.*

Among the numerous other substances proposed as disinfectants, or for dressing wounds, the following have not afforded satisfactory results:

Chlorate of potash,—mixed with clay or kaolin (for example, 10 parts of chlorate to 90 parts of white clay or fine sand) which was proposed as an absolute disinfectant, neither disinfected nor absorbed the pus of fetid wounds. The mixture would be in any case much more costly than coal-tar and plaster and certainly less efficacious.

Whites of eggs,—mixed with chalk and applied to wounds, previously oiled, succeeded no better than simple-cerate.

* The inefficiency of sulphate of lime as a general disinfecting agent when used by itself may be readily demonstrated by the following experiment which is of interest in view of the fact that a belief in the utility of gypsum as a deodorizer appears to be widely spread among recent writers. For that matter we are told by Paulet (*Comptes Rendus*, xlix, 199) that during the last 25 years more than fifty authors of processes of disinfection have announced, each as he believed for the first time, the use of plaster as a means of disinfection.

If a mixture of about equal volumes of powdered gypsum and fresh urine be introduced into a small phial, the mixture placed in a warm room and thoroughly shaken several times a day until the urine has become putrid, it will be observed that an exceedingly disagreeable odor will be developed, differing from that of ordinary stale urine inasmuch as it is unalloyed with the odor of ammonia. For the complete success of this experiment it is important that a large excess of sulphate of lime should be present and that the mixture should be frequently agitated, else the whole of the carbonate of ammonia will not be decomposed and will tend to mitigate the fetor of the special odor of the putrid urine. So far from disinfecting in this case the sulphate of lime really destroys a deodorizing, or at least a masking agent, ammonia; leaving free,—purified as it were, and unadulterated, an odor, the peculiar offensiveness of which is remarkable. Sulphate of iron being substituted for gypsum in this experiment afforded a somewhat similar result, although the odor obtained was a trifle less insufferable than that of the experiments with sulphate of lime. It should be here mentioned that the odors in question were in no instance contaminated with sulphuretted hydrogen,—as was ascertained by careful trials. F. H. STORER.

Powdered sugar.—When employed in layers upon ulcers forms crusts, beneath which the suppurations accumulate and hinder the process of healing.

Cherry-laurel water, glycerine and cellulose.—According to Antier, glycerine mixed with equal parts of cherry-laurel water forms a valuable absorbent or disinfectant to be applied as a lotion or injection. This mixture converted into pomade by mixing it with powdered almonds was also proposed as a topical application for all kinds of wounds. But in the hands of the committee neither the liquor nor the pomade by themselves or mixed with kaolin produced any effect more marked than that of lead-cerate and other anti-putrid or detergent solutions already in use.

The members of another group of disinfectants are worthy, in various degrees of consideration.

Among these charcoal appears in the front rank.—Surgeons have long regarded it as one of the best antiseptics known. Confined between pieces of linen according to the process of Malapert and Pichot it is more readily applied than when used as powder directly upon wounds; but the mixture of coal-tar and plaster, which disinfects still better and is more cleanly, is susceptible of a simpler and a more general application.

Coke of Boghead coal,—in powder as proposed by Moride* like carbon

* In view of the claim of Moride (*Comptes Rendus*, xlix, 242) as well as from its general interest the following extract from a report made to the British Secretary of War by Lewis Thompson (London Journal of Gas Lighting, Water Supply and Sanitary Improvement, 1856, v, 11) may here be cited.

Mr. Thompson states that he has instituted a set of experiments having a purely money basis as their exponent.—The articles enumerated were each employed until they practically deodorized one uniform quantity of the same mass of putrid sewage and the money value of the proportions thus used was deduced either from a broker's price-list or, where this failed to give the requisite information, by special inquiry from a wholesale dealer. The amount of sewage operated upon in each experiment was half a gallon taken from a single tank which had been recently filled out of a large and very offensive ditch or open sewer. Two indications of the progress of the disinfection were had recourse to in these experiments; one with paper dipped in sugar of lead which gradually ceased to become brown as the deodorizing agent was added in successive portions; the other had reference to the discontinuance of any offensive smell; and the attainment of this last condition was regarded as the termination of each experiment.

By this means he was enabled to draw up the subjoined table which shows at a glance the comparative cost of executing the same amount of deodorizing work with each agent on the supposition that Boghead charcoal can be had at the rate of $3·00 [=12*s.*] per ton.

Table showing the cost of Purifying one uniform Quality of Feculent Sewage by the several Articles mentioned.

Boghead charcoal (coke),	$3·00
Nitric acid,	8·50
Black oxyd of manganese,	9·25
Chlorid of lime,	10·75
Peat charcoal,	11·00
Subchlorid of iron (imperfect),	11·25
Animal charcoal,	16·75
Chlorid of manganese (imperfect),	17·50
Bichlorid of mercury,	18·00
Impure chlorid of zinc in damp powder,	26·00
Chlorid of zinc in solution as patented by Sir Wm. Burnett,	37·00
Sulphate of copper,	39·00

The sulphates of zinc, iron, and alumina; common gypsum; sulphuric, sulphur-

when employed comparatively with coal-tar and plaster, alternately upon the same patients, proved to be less efficacious, less convenient and more disagreeable than the latter.

Mixed plaster and charcoal,—proposed by Herpin of Metz, irritates the wounds, disinfects badly, and soils everything it touches.

Carbonic acid,—proposed by the same author, appears to the committee to be too difficult of application in practice, though theoretically founded upon important analogies.

Bituminous Water of Visos—proposed by Manne, and the mud of rivers used as a poultice by Desmartis, do not appear to be susceptible of being substituted for the mixture of Corne and Demeaux.

The following substances have long ago acquired a place, each in its own way, in the class of disinfectants.

Tincture of iodine has been employed as an antiseptic by hospital surgeons since 1823. By modifying the surfaces to which it is applied, it usually improves the appearance of the pus, lessens its acridity, and is, to a certain extent, antagonistic to putrid infections. It disinfects, however, only incompletely, causes severe pain when applied to open wounds, and would be expensive if used on a large scale; finally, the odor of iodine is neither agreeable nor unattended by inconveniences.

Perchloride of iron has been used for some twelve years in hospitals as an antiseptic and as a means of modifying certain wounds, and putrid or sanguineous foci.—Without diffusing the disagreeable odor of tincture of iodine, it has, like the latter, the fault of disinfecting badly, of causing much pain, and of acting violently upon the diseased tissues, besides injuring the cloths which are soaked in it even more than is the case with the coal-tar and plaster.

Both iodine and the salt of iron just mentioned, are in fact agents of another order; they have rendered, and do still render important services. They are certainly well worth preserving, but should not be compared with the mixture of coal-tar and plaster.

Nitrate of lead,* *Creosote*, and other substances which have been proposed at one time or another, have not realized the expectations of their inventors; their price has been too great, their employment required too much care, or their action has not been sufficiently certain that they could be advantageously used in practice. There is, nevertheless, one of these which deserves special mention, viz., *chlorine*. Ever since Guyton Morveau demonstrated the true action of muriatic acid upon putrefying animal matters, the efficacy of chlorine has been tested in almost innumerable ways. Solutions of chlorine, of "chlorid of soda," and of "chlorid of lime," have rendered signal services to medicine and in the cause of public

ous, and muriatic acids; peroxyd of iron, highly dried clay, litharge, and saw-dust were found imperfect even when very large quantities were employed.

Arsenious acid and creosote on the contrary, were very active; but the danger of a subsequent evolution of arseniuretted hydrogen in the first case, and the difficulty of diffusing an oily fluid like creosote in the second, seemed to interdict the use of these substances. F. H. S.

* [An excellent, though somewhat expensive "disinfecting fluid" (Ledoyen's), which was quite extensively used in this country a few years since, consisted, according to analyses of F. E. Holyoke, of an aqueous solution of this salt.—F. H. S.]

health, especially since Labarraque, some thirty years since, indicated an improved method of employing them. But the odor of chlorine, disagreeable in itself, is neither easily borne nor devoid of inconveniences. Wounds, moreover, hardly accommodate themselves to it any better than the sense of smell, whenever somewhat large doses of it are required.

Chlorinated Sponge.—The idea of applying sponges saturated with chlorinated solutions, directly upon purulent or gangrenous wounds, as suggested by Hervieux, appears to be excellent for certain cases. Such sponges, renewed several times per day, absorb the pus gradually as it forms better than anything else, and disinfect the wound very well. Unfortunately, chlorine rapidly alters or destroys the sponges and soon causes undue irritation. While this method, therefore, is an excellent one for cleaning certain gangrenous and sinuous wounds, it is, nevertheless, inferior in most instances to the mixture of coal-tar and plaster.

Subnitrate of bismuth—suggested by Frémy as an absorbent and disinfectant, was applied to a large number of wounds. Upon large cavernous cancers it disinfected somewhat better than Peruvian bark, charcoal, or chlorate of potash, but less than the coal-tar and plaster. By its use, however, several bad looking wounds were cleansed quite rapidly. Since it causes no pain or irritation, and since it neither soils the skin nor the clothes, the subnitrate of bismuth is preferable to a multitude of other antiseptic powders; but it is useful rather as a solidifier (*incarnatif*), or dryer, than as an absorbent or disinfectant.

In their *résumé* the committee affirm:

I. That coal-tar mixed with plaster, according to the formula of Corne, (see this Journal, xxviii, 426), can disinfect putrefying organic matters. Mixed with alvine dejections this powder destroys their odor, and leads one to hope that by its aid profound reforms in the present system of maintaining and clearing out cess-pools, &c., may some day be brought about. For this purpose, ordinary earth, coal-ashes, or sand may be substituted for the plaster, as Cabanes prefers, being at least equally efficacious.

II. In therapeutics the coal-tar and plaster has fulfilled only a part of its promises. As a disinfectant in the dissecting-room, upon the folds of bandages,—everywhere where there is infectious matter, its qualities are incontestable. This is also true as regards putrid or gangrenous foci, fetid suppurations, sanious wounds, ichorous putrilagenous cavities, hospital gangrene, &c.; but upon acute and exposed wounds, or upon ordinary wounds or ulcers, other topical applications are preferable to it.

III. Used with lint upon cloths, with pomades or cerate, it has afforded no useful result, and nothing has occurred to prove that when administered internally it has produced the least benefit.

IV. As an absorbent it leaves much to be desired, although it is not entirely devoid of action. When applied as a poultice, in particular, it absorbs very incompletely. For that matter the mixtures of coal-tar with earth or with other powders, absorb still less than the mixture of Corne and Demeaux, and are scarcely at all applicable in therapeutics. In this connection it must not be forgotten that the morbid liquids, and particularly pus, are very different from water. A substance like plaster, for example, which absorbs water strongly, might be incapable of absorbing pus. It is nevertheless true, that as an absorbent, the mixture of coal-

tar and plaster, either as powder or as poultice, is of some use upon fetid and putrid wounds or suppurations.

V. Cellulose, glycerine and cherry-laurel water; chlorate of potash mixed with talc, clay, marl or kaolin, are neither sufficiently efficacious, nor in application are they convenient enough to be retained in practice.

VI. The mixture of saponine and coal-tar does not appear to be preferable for dressing wounds to many other liquids already known,—tincture of aloes for example. The same may be said of the mixed coal-tar and charcoal of Herpin; nor does it seem as if carbonic acid should be used, unless some improved method of applying it can be devised.

VII. The Boghead residue would be useful only in lack of coal-tar and plaster. While charcoal in porous envelopes does not mould itself to cavernous and sinuous wounds with sufficient readiness to come into general practice.

VIII. From its low price, and by its action, at once mild, absorbent, and disinfectant, as well as by its drying properties, the subnitrate of bismuth will render important services in default of the mixture of coal-tar and plaster. It is even preferable to this when the wounds are accompanied or surrounded with heat or irritation.

IX. Tincture of iodine and perchlorid of iron act rather by modifying the surfaces of wounds and of purulent foci, than as absorbents or disinfectants. They have their special applications in surgery, but agents of this sort are not comparable with the mixed coal-tar and plaster.

X. Sponges soaked in chlorinated water can also render good service upon pale, burrowing sores and upon gangrenous foci.

We have occupied ourselves, say the committee, only with the practical or experimental side of the question. A discussion of its theoretical or chemical bearings would have carried us too far. Moreover, since the authors of the different communications which have been submitted to us have themselves neglected this for the most part, it has seemed to us useless to treat of it at present; whether it be the phenic acid or rosolic, or brunolic acid, or the anilin, picolin, etc., of the coal-tar, which disinfects, is in reality of but little importance. Science will inform us of this some day no doubt; for the moment we have merely to ascertain whether or no the various disinfectants which are brought to us do really disinfect.

After citing the labors of various persons who have proposed methods of disinfection, the committee go on to say: "M. Corne, and the authors indicated above, occupied themselves only with the disinfection and the solidification of animal matter, having in view the preparation of manures. * * * * It is M. Demeaux who appears first to have had the thought of applying to fetid wounds, in surgical practice, the powder invented, or adopted and extolled, by his neighbor. In addition, it is evident that here, as is the case with so many other complex facts with which science is enriched, there is, so to say, neither invention nor priority for any one. The subject has been worked upon for more than a century—a multitude of savans having competed with each other in studying it. Little by little the evolution of the discovery has been effected. M. Corne disengaged it from its gangue a little better than his predecessors, and Demeaux, knowing, perhaps, that from time immemorial sailors and the inhabitants of certain southern countries often dress their wounds

with tar; that tar water and pomades of tar are frequently employed in medicine, has extended its applications to therapeutics."

"Many other efforts are still necessary. In point of fact the results thus far obtained are merely rough outlines,—only first trials. So long as the world at large is not in possession of a simple, easy, and economical method, accessible to every one, which shall be capable of disinfecting immediately, and without inconvenience on the large or small scale, dejections and filth of all kinds, in dwellings as well as in privies or slaughter-houses; in dissecting-rooms and the like, as well as in the sick room, upon wounds, improvements will still be wanted; there will yet be room for new attempts. While recording those of to-day and those of yesterday upon the road already traversed, let us be careful not to diminish the ardor of the laborers in the future, who will finally endow civilization with a complete and general disinfection."

Finally, certain indispensable precautions must be followed, in order to obtain from the process of Corne and Demeaux its proper effect. It is evident, from having neglected some of these precautions, that different experimenters have been led to believe that the method is useless. Fine moulding plaster, and not the common article, should be employed. The coal-tar, which is mixed with it in the proportion of 2 to 4 parts to a hundred, by triturating or grinding, ought to impart to it a gray tint, without destroying its dry, pulverulent condition. Objects to be disinfected should be rolled in this powder until each point upon their surfaces has been brought in contact with it. Gangrenous or putrid sores should be covered with thick layers of it, by handfuls, several times per day. If one is treating pus, blood, dejections, or the like, enough of the powder should be added to form a paste of the mass, taking care to replace the first layer of powder by another as soon as it no longer absorbs any more. Mixed with oil to the consistence of a thick pap, it forms poultices of convenient application, if they are made thick and broad.

Within the limits which have been indicated the mixture of coal-tar and plaster is a good disinfectant, and may be recommended for use in domestic economy as well as in hospitals. "What we have ourselves seen leaves no uncertainty of the reality of this property, nor of the possibility of its application." * * * * * It remains only to draw from it reasonable, practical consequences, either taking the fact as it is, or by modifying and perfecting it.—*Comptes Rendus*, l, 279.

[For corroborating testimony, received by the committee from various sources, the reader is referred to their report in question. Numerous other articles upon the subject, by different authors, may also be found in vol. xlix of the *Comptes Rendus*.—F. H. S.]

7. *Decoloration of Indigo by Sesquioxyd of Iron.*—[In the May number of this Journal we took occasion to maintain that the very interesting fact of the power of ferric salts to bleach solutions of indigo was first observed by Prof. H. Wurtz. Since then we have accidentally learned that this claim was erroneous, as will appear from the following statement made by Wœhler some twenty years since. "When a solution of indigo in sulphuric acid is mixed with salts of the sesquioxyd of iron and heated, it will be decolorized precisely as it would be by nitric acid."—(Annalen der Chemie und Pharmacie, 1840, xxxiv, 235; see also Gmelin's Handbook (Cavendish Soc. Edit.), xiii, 59.—F. H. S.]

II. GEOLOGY.

1. *On Some Points in Chemical Geology*; by T. STERRY HUNT, F.R.S. (Read before the *Geol. Society of London*, January 5th, 1859--published in the *Quarterly Journal* of the Society for November, 1859—and reprinted, with additional notes by the author, in the *Canadian Naturalist* for January, 1860.).—In this paper the author discusses a number of questions which lie at the foundation of a true history of the chemistry of the earth's crust, and gives farther developments to some of his peculiar views, which were for the most part, first enunciated in this Journal. In regard to the metamorphism of sedimentary deposits, *i. e.*, the conversion of sands, clays and marls into crystalline stratified rocks, the author, after distinguishing between local and normal metamorphism, insists upon the frequent interstratification of unchanged fossiliferous limestones among crystalline schists as evidence that heat has not been the only agent in the metamorphism, which has moreover been effected at temperatures not very elevated, and by the intervention of alkaline solutions, in the absence of which, sediments may be heated to the same degree without change.

The first announcement of this view will be found in this Journal for May, 1857 (vol. xxiii, p. 407), where the author, after describing some experiments with the alkaline silicates, expresses the opinion that "we have here the explanation of rock metamorphism in general." Farther inquiries into the action of the soluble alkaline silicates will be found in this Journal for March and May, 1858 (vol. xxv, pp. 287–437), where the subsequent experiments of Daubrée are cited in confirmation and extension of Mr. Hunt's theory of the normal metamorphism of sediments at comparatively low temperatures by the intervention of alkaline carbonates and silicates, which may be either liberated by the decomposition of the sediments themselves or derived from adjacent strata. These salts in solution permit the crystallization of feldspars and micas, or when alkaline bases are present only in smaller quantity, of kyanite, andalusite and staurotide, while by the intervention of protoxyd bases, garnet, epidote, chloritoid and chlorite are formed, and in the absence of the argillaceous element, pyroxene, olivine, serpentine and talc. In a subsequent note the author has however alluded to the probable direct formation of certain silicates of magnesia and lime, in open basins at the earth's surface and by reactions at the ordinary temperature. This Journal, March, 1860 (xxix, 284).

In the second place the author discusses the relations of plutonic to metamorphic sedimentary rocks, and concludes that the latter, becoming plastic under the influence of water and heat, may be displaced by disturbance and pressure, thus taking the form of intrusive rocks. Sediments altered *in situ* he distinguishes as indigenous, and those displaced as exotic plutonic rocks. The conclusions of Scrope, Scheerer and Elie de Beaumont, supported by the late observations of Daubrée and Sorby, as to the aqueo-igneous fusion of these rocks, are fully admitted.

In the third place the author discusses the theories of Phillips, Bunsen and Durocher, as to the origin of intrusive rocks, and rejecting the notion that these are derived from the supposed fluid interior of the earth, re-

gards them as in all cases, fused and displaced sediments. He proceeds to show that the action of waters removing from permeable strata their soda, lime and magnesia, brings these to the composition of granitic rocks, while the finer and less permeable sediments, retaining their protoxyd bases, give rise by subsequent alterations to basic rocks with triclinic feldspars and pyroxene. Soda being preëminently the soluble alkali, has been gradually removed from disintegrated feldspathic rocks under the influence of water and carbonic acid, and the carbonate of soda thus formed has by its reaction with the lime salts of the ancient ocean, given rise to sea-salt and to the carbonate of lime with which the limestones have been built up. The aluminous silicate set free in the decomposition of the feldspars is thus the equivalent of the earthy carbonates and sea-salt which are formed. Hence we find that in the oldest known crystalline rocks, those of the Laurentian series, soda feldspars are abundant, micaceous schists rare, and argillites or silicates of alumina deficient in alkali are unknown, while in higher formations, argillites and schists with kyanite, chiastolite and staurotide abound, as well as chlorite, chloritoid, muscovite, garnet, epidote, etc., showing a great excess of aluminous silicate over the alkali required to form feldspars.

These views have already appeared in a communication from the author in this Journal (vol. xxv, p. 436) where also the action of organic matters in deoxydizing, dissolving and removing oxyd of iron from certain strata to be accumulated in others, is discussed and illustrated by a consideration of the iron deposits of various ages. The existence of beds of iron ore in the Laurentian rocks, not less than the graphite and metallic sulphurets which these contain, is by the author regarded as evidence that organic life existed at the period when these rocks were deposited. As Mr. Hunt has elsewhere explained, he supposes the condition of the cooling globe to have been one of thorough oxydation, and regards all processes of reduction or deoxydation as dependent either directly or indirectly upon organic life. In regard to iron oxyd however he remarks that its solution may sometimes be due to mineral acids, from volcanic or other sources; such solutions, and others from the oxydation of pyrites, may be decomposed by alkaline or earthy carbonates and give rise to iron deposits. A similar process with aluminous solutions will serve to explain the origin of corundum, beds of emery, and aluminous iron ores.

In regard to the agency of organic matters in the formation of iron deposits we may here remark that a reviewer in this Journal (xxv, 245) in noticing Mr. Hunt's observations on this subject contained in his Geological Report for 1856, writes as if Mr. H. had appropriated the views of Bischof. In truth neither Bischof nor Hunt ever claimed any originality in bringing forward a principle which has long been understood, and which they have only attempted to extend and develope. We may here observe that the same reviewer fails to apprehend Mr. Hunt's views on the formation of crystalline rocks, when he says that Bischof and Hunt agree in supposing all rocks to be formed by chemical agencies in the presence of water, and that therefore the latter cannot claim originality. Now upon the point in question there is little or no affinity between the views of the two writers, for the simple reason that Bischof seems never for a moment to apprehend the nature of the great problem which Mr. Hunt has under-

taken to solve, but with Dana regards normal metamorphism as pseudomorphism on a grand scale. The ingenious speculations of Bischof and others on the possible alteration of mineral species by the action of various saline and alkaline solutions may pass for what they are worth, although we are satisfied that by far the greater part of the so-called cases of pseudomorphism in silicates are purely imaginary, and when real are but local and accidental phenomena. Bischof's notion of the pseudomorphism of silicates like feldspars and pyroxene presupposes the existence of crystalline rocks, whose generation this neptunist never attempts to explain, but takes his starting point from a plutonic basis.

The problem to be solved, as we have elsewhere insisted, is the conversion of sands, clays and marls, (consisting of silica, silicates of alumina, carbonates of lime, magnesia and oxyd of iron, derived by chemical and mechanical agencies from the ocean waters and pre-existing crystalline rocks,) into aggregations of crystalline silicates. These metamorphic rocks, once formed, are liable to alteration only by local and superficial agencies, and are not, like the tissues of a living organism, subject to incessant transformations, the pseudomorphism of Bischof and Dana. As yet, Mr. Hunt is the only one who has attempted a rational explanation, based on experiments, of the problem of the conversion of sedimentary strata into crystalline rocks, and his views, whether true or false, are to be judged by themselves, and not by comparison with those of Bischof or any other writer. Among the geologists who since the time of Hutton, have best comprehended the nature of the problem of rock metamorphism, are Boué, Virlet and Delanoüe. We hope at an early day to discuss in the pages of this Journal, the question of mineral pseudomorphism, as well as the history and theory of metamorphism.

Passing from plutonic to volcanic phenomena, Mr. Hunt proceeds to develope the views of Babbage and Herschel as to the effects of the internal heat upon deeply buried sediments. Babbage has shown that the expansion of the sedimentary rocks by heat may cause great vertical movements, while in the subsequent fusion of the heated sediments Herschel finds an explanation of volcanic phenomena. These views the author adopts, and enters into a consideration of the relations which must take place between silica and silicates, carbonates, sulphates, chlorids and organic matters in the presence of water at an elevated temperature. To azotized organic substances and to the ammonia condensed in argillaceous strata, he ascribes, with Bischof, the ammoniacal salts of volcanoes.

In considering the metamorphism of the strata, which must always precede volcanic action, we are not to lose sight of a process which will, in its results, be the reverse of that insisted upon by Babbage. It is the great contraction which must result, not only from the solidification of the porous sediments, but from the condensation attendant upon chemical combination, by which they are converted into silicates of high specific gravity, such as pyroxene, garnet, epidote, chloritoid and chiastolite. In this way, as remarked by Mr. Hunt in his lectures before the Smithsonian Institution at Washington last winter, we may realize, to a certain extent, Elie de Beaumont's notion of a shrinking of the earth's nucleus, and find an explanation of many phenomena of subsidence and corrugation, although with Herschel we are to attribute these for the most part to "the disturb-

ance of the equilibrium of pressure consequent upon the transfer of sediments while the yielding surface reposes upon a mass of matter partly liquid and partly solid."

We conclude with the following extracts, and with two notes appended to the Canadian reprint of Mr. Hunt's papers, in one of which he calls attention to the remarkable work of Keferstein, whose ingenious views, too much in advance of his time, have hitherto been overlooked.

"The metamorphism of sediments *in situ*, their displacement in a pasty condition from igneo-aqueous fusion as plutonic rocks, and their ejection as lavas with attendant gases and vapors are then all results of the same cause, and depend upon the differences in the chemical composition of the sediments, the temperature, and the depth to which they are buried: while the unstratified nucleus of the earth, which is doubtless anhydrous, and according to the calculations of Messrs. Hopkins and Hennessey, probably solid to a great depth, intervenes in the phenomena under consideration only as a source of heat." *

"The volcanic phenomena of the present day appear, so far as I am aware, to be confined to regions covered by the more recent secondary and tertiary deposits, which we may suppose the central heat to be still penetrating (as shown by Mr. Babbage), a process which has long since ceased in the palæozoic regions. Both normal metamorphism and volcanic action are generally connected with elevations and foldings of the earth's crust, all of which phenomena we conceive to have a common cause, and to depend upon the accumulation of sediments and the subsidence consequent thereon, as maintained by Mr. James Hall in his theory of mountains. The mechanical deposits of great thickness are made up of coarse and heavy sediments, and by their alteration yield hard and resisting rocks; so that subsequent elevation and denudation will expose these contorted and altered strata in the form of mountain chains. Thus the Appalachians of North America mark the direction and extent of the great accumulation of sediments by the oceanic currents during the whole

* "The notion that volcanic phenomena have their seat in the sedimentary formations of the earth's crust, and are dependent upon the combustion of organic matters, is, as Humboldt remarks, one which belongs to the infancy of geognosy. In 1834 Christian Keferstein published his *Naturgeschichte des Erdkörpers*, in which he maintains that all crystalline non-stratified rocks, from granite to lava, are products of the transformation of sedimentary strata, in part very recent, and that there is no well-defined line to be drawn between neptunian and volcanic rocks, since they pass into each other. Volcanic phenomena according to him have their origin, not in an igneous fluid centre, nor an oxydizing metallic nucleus, but in known sedimentary formations, where they are the result of a peculiar process of fermentation, which crystallizes and arranges in new forms the elements of the sedimentary strata, with evolution of heat as an accompaniment of the chemical process. (*Naturgeschichte*, vol. i, p. 109, also *Bull. Soc. Géol. de France* (1) vol. vii, p. 197).

These remarkable conclusions were unknown to me at the time of writing this paper, and seem indeed to have been entirely overlooked by geological writers; they are, as will be seen, in many respects an anticipation of the views of Herschel and my own; although in rejecting the influence of an incandescent nucleus as a source of heat, he has, as I conceive, excluded the exciting cause of that chemical change, which he has not inaptly described as a process of fermentation, and which is the source of all volcanic and plutonic phenomena. See in this connection my paper *On the Theory of Igneous Rocks and Volcanoes*, in the Canadian Journal for May, 1858."

palæozoic period; and the upper portions of these having been removed by subsequent denudation, we find the inferior members of the series transformed into crystalline stratified rocks."*

III. BOTANY AND ZOOLOGY.

1. *Flora of the Southern United States, containing abridged descriptions of the Flowering Plants and Ferns of Tennessee, North and South Carolina, Georgia, Alabama, Mississippi, and Florida; arranged according to the Natural System;* by A. W. Chapman, M.D. (The Ferns by Daniel C. Eaton.) New York: Ivison, Phinney & Co. 1860. pp. 621, small 8vo.—The first thing that strikes our attention as we open this volume, is its neat and tasteful typography. It is a decided advance upon its counterpart, Gray's Manual for the Northern States, in this respect, and indeed is the handsomest volume of the kind we know of. It is only just to add that the book was produced by the University Press of Welch, Bigelow & Co., Cambridge. The matter of the volume is, we trust, as good as its form. It well supplies a long-felt and pressing want, and gives to schools and colleges, and to botanical students generally at the South, a work which is for that district what Gray's Manual is for the northern section of our common country. Having said this, modesty prevents more particular eulogium. If experience annually shows that the work with which this volume is compared is not yet perfect, but still requires many minor emendations, notwithstanding long pains-taking and

* "The theory that volcanic mountains have been formed by a sudden local elevation or tumefaction of previously horizontal deposits of lava and other volcanic rocks, in opposition to the view of the older geologists who supposed them to have been built up by the accumulation of successive eruptions, although supported by Humboldt, Von Buch, and Elie de Beaumont, has been from the first opposed by Cordier, Constant Prevost, Scrope and Lyell. (See Scrope, *Geol. Journal*, vol. xii, p. 326, and vol. xv, p. 500; also Lyell, *Philos. Trans.*, part 2, vol. cxlviii, p. 703, for 1858.) In these we think will be found a thorough refutation of the elevation hypothesis and a vindication of the ancient theory.

This notion of paroxysmal upheaval once admitted for volcanoes was next applied to mountains which, like the Alps and Pyrenees, are composed of neptunian strata. Against this view, however, we find De Montlosier in 1832 maintaining that such mountains are to be regarded as the remnants of former continents which have been cut away by denudation, and that the inversions and disturbances often met with in the structure of mountains are to be regarded only as local accidents. (*Bul. Soc. Geol.*, (1) vol. ii, p. 438, vol. iii, p. 215.)

Similar views were developed by Prof. James Hall in his address before the American Association for the Advancement of Science, at Montreal in August, 1858. This address has not been published, but these views are reproduced in the first volume of his *Report on the Geology of Iowa*, p. 41. Mr. Hall there insists upon the conditions which in the ancient seas, gave rise to great accumulations of sediment along certain lines, and asserts that to this great thickness of strata, whether horizontal or inclined, we are to ascribe the mountainous features of northeastern America as compared with the Mississippi valley. Mountain heights are due to original deposition and subsequent continental elevation, and not to local upheavals or foldings, which on the contrary, give rise to lines of weakness, and favor erosion, so that the lower rocks become exposed in anticlinal valleys, while the intermediate mountains are found to be capped with newer strata.

In like manner J. P. Lesley asserts that 'mountains are but fragments of the upper layers of the earth's crust,' lying in synclinals and preserved from the general denudation and translation. (*Iron Manufacturer's Guide*, 1859, p. 53.) See also his admirable little volume entitled *Coal and its Topography*."

repeated revisions, it may be expected that equal experience will reveal similar imperfections in the new and untried work. None but a practised teacher can tell beforehand where the pupil, or the student without a teacher will encounter obstacles, and the most experienced can only partially anticipate them. They must be found out by trial, and be corrected in new issues,—for which electrotyping offers great facilities. For the young student the Artificial Analysis of the Natural Orders is practically the key to the whole thing; a perfect key of this sort was never made at one trial; in fact most keys in such works fail very largely. So we may safely hazard the prediction that Dr. Chapman's artificial analysis will need emendation in a future edition. We are bound to add, however, that a half dozen of trials has resulted in only one failure.

This book is wanted by botanists as well as by students, and we think they will be well pleased with it. Its merits are manifold; its deficiencies are either such as are natural if not unavoidable under the circumstances of its production, or such as pertain to all works of the kind, in which knowledge is progressive, the records of this knowledge widely dispersed, and the facts to be observed and digested into order almost infinitely numerous. An introduction gives a good condensed sketch of the Elements of Botany, and a Glossary of Botanical Terms, so that the book can be used independently; though such a book as the First Lessons in Botany ought to precede and accompany its use by the student. Very appropriately is this volume dedicated to one of the worthiest botanists of the Southern States, the Rev. Dr. Curtis. Now that the Southern Atlantic states are provided with a good Flora of their own, we trust that botany will receive a new impulse, both as a scientific pursuit and as a branch of education, in that favored region. A. G.

2. *Synopsis of Dalbergieæ, a Tribe of Leguminosæ*; by George Bentham, Esq.—This is a (botanical) supplement to the fourth volume of the Journal of the Proceedings of the Linnæan Society, a critical account of this group of plants, and a technical synopsis of the genera and species, —of much importance therefore to the systematic botanist. Since its publication, better specimens have been examined of one of Fendler's Venezuelan plants, No. 2223, referred to on p. 17; and Mr. Bentham finds this a new generic type, *Fissicalyx*. A. G.

3. *Reports on the Natural History, Climate, and Physical Geography of Minnesota, Nebraska, Washington, and Oregon Territories; made in connection with the Survey of a Pacific Railroad Route, along the forty-seventh and forty-ninth parallels of latitude, in* 1853–4–5–6, *under the command of Governor* I. I. Stevens, *of Washington Territory;* by G. Suckley, M.D., and J. G. Cooper, M.D., Surgeons and Naturalists to the Expedition. 4to, pp. 399, 26, with 65 plates, and isothermal charts, &c. New York, Bailliere Brothers.—This is a separate issue of the 12th volume of the Pacific Railroad Exploration Reports, containing all that relates to science in the report of Gov. Stevens' survey of the northern proposed route, and also a preface and other additional matter by the enterprising authors, Drs. Cooper and Suckley. This issue is, we understand, rendered necessary by the cutting down of the government edition in the later volumes of the series to a much diminished number. The authors have taken this opportunity not merely to add new matter, and

make certain emendations, but also to insert four or five pages of errata, correcting typographical errors, perhaps a tithe of those contained in the volume. This remark does not at all militate against the statement that in this volume "there are fewer errors than is usual in similar government publications." We are satisfied of the truth of this. The mass of scientific reports published by Congress and printed at Washington are marred beyond all endurance by want of proof-reading, or perhaps of proof-correction,—for in some instances proofs were furnished and revised, but the corrections were never made. The folly of the late system having now been exposed in other and more considerable respects, we may hope for the inauguration of a much better and more economical plan.

The first chapter of the present very interesting volume (whether peculiar to this edition or no is more than we can now ascertain) is separately paged, and is devoted to the meteorology and climate of Nebraska and Washington Territories. The Botanical Report, which comes next in order, consists, 1. of a very interesting and useful general sketch of the botany of the route, in reference to the character of the vegetation, geographical distribution, &c., mingled with zoological and climatal observations. 2. A catalogue of the plants collected east of the Rocky Mountains, containing three before undescribed species and one new (Chenopodiaceous) genus,—all of which are characterized by Dr. Torrey, as will be seen, although the list was drawn up by Dr. Gray. This portion is illustrated by four excellent plates, one of which is devoted to the *Endolepis Suckleyi* of Torrey, the new genus referred to. 3. Catalogue of Plants collected in Washington Territory; with observations, &c., drawn up by Dr. Cooper himself, including notes, characters, &c., supplied by Dr. Gray, Dr. Torrey, and Prof. Thurber; with two plates.

The remainder and most striking part of the volume is the Zoological Report; that on the Insects by Dr. Leconte; on the Mammals by Dr. Cooper, Dr. Suckley and Mr. Gibbs; on the Birds, by Drs. Cooper and Suckley; on the Reptiles, by Dr. Cooper; on the Fishes by Dr. Suckley; on the Mollusca by Wm. Cooper, Esq. (a veteran naturalist whom we gladly welcome back to active labor); on the Crustacea by Dr. Cooper. The zoological plates are many and truly beautiful; those of the birds are colored. The volume sells for ten dollars; and this small separate issue will doubtless be taken up at once, to complete the sets of the Pacific Railroad Reports. A. G.

4. *Potamogeton crispus* L. was introduced into the North American flora by Pursh, and said to occur from "Canada to Virginia." Dr. Torrey, in his Flora of the Northern States, mentions it as from Lake George; but as he omits it from his recent Flora of the State of New York, we infer that there was some mistake in the first instance. Prof. Tuckerman, who has paid great attention to this difficult genus, not having found *P. crispus* in this country, and not having ourselves met with it, the species was excluded from the Manual of the Botany of the Northern United States; but a remark was added in the second edition of this work, that Mr. Tuckerman had seen a specimen in some European herbarium purporting to come from Delaware. It may also be noted that this species has for many years been growing in a pool in the Botanic Garden at Cambridge,—where in fact it cannot be got rid of,—and there

is a tradition that it was introduced into the pool by Mr. Nuttall. Last year Mr Edward Tatnall, an intelligent botanist and horticulturist, detected the plant in question in Delaware, in the vicinity of Wilmington, and this season he finds it to occur abundantly, under circumstances which give it a good claim to be regarded as indigenous. So *Potamogeton crispus* must be restored to our flora. If really indigenous it probably occurs in other stations. Its early flowering and fruiting, compared with the other species (viz., blossoming in May) may have led to its being overlooked; but the species is probably local in this country. It is, however, so vigorous and so difficult of eradication where it is established, so likely therefore to hold its own or to extend, that, if not detected elsewhere, we may believe that it was recently imported into this country, as another water-weed, the *Anacharis* of North America, was into England, where it has spread prodigiously with a few years. A. G.

5. *Marsilea quadrifolia*, L.—Aquatic plants, especially those of low type, are in general so widely diffused geographically, that the absence from North America of the above named plant,—so common throughout the northern part of the Old World,—has always seemed rather exceptional. We have now to announce its actual occurrence here. It has just been discovered on the muddy borders of a pond in Litchfield, Connecticut, by Dr. Timothy F. Allen. This adds another instance of the apparently extremely local occurrence in this country of a common European species, of which *Scolopendrium officinarum* and *Subularia aquatica* are cases in point. As it is not likely that this Marsilea was created for Litchfield pond, or for any other few localities, if such there be, in this country, such plants must be regarded as of recent and casual introduction—which is most improbable—or else as species once diffused over the country, but now on the verge of extinction from this flora,—a view which chimes in with other inferences about geographical distribution.

A. G.

6. *Catalogue of the Acanthopterygian Fishes in the collection of the British Museum;* by Dr. A. Gunther. 8vo, pp. 524. London, 1859. —In this volume all the fishes in the British Museum, belonging to the families mentioned are described, and abstracts are given of the characters of many others, not in that collection. The work must be therefore, useful to naturalists, and especially to students, as the author has been quite diligent in collecting the indications of species from all sources. Considerable discretion must however be used in consulting it, as the author appears to have considered many species that he had not himself seen, as being very "doubtful" or as identical with some known to him. In his work, our American fresh-water fishes are especially in great disorder. None of the genera recently established by Agassiz and Girard are adopted. The *Calliuri* are placed partly in *Centrarchus* and partly in *Brythus*. The *Grystes* are distributed in *Centrarchus* and *Huro* as well as *Grystes*. If recent American works had been consulted these errors would not have occurred. Some species are regarded as identical which have no close relationship to each other, as *Pomotis falax*, B. and S., and *P. rubricanda* Storer. In many other points Dr. Gunther differs from the best Ichthyologists who have hitherto treated of the order. Thirteen genera and forty species are described as new. W. S.

IV. ASTRONOMY.

1. *First Comet of* 1860.—(Gould's Astron. Jour. No. 134).—" A letter from Mr. Liais, Director of the Brazilian Coast Survey, to Prof. Peters, published in the *Astronomische Nachrichten*, No. 1248, announces the discovery, February 26th, at Olinda, Brazil, of a faint double comet, near the star μ Doradûs. The larger portion preceded; it was brighter on the side toward the sun, and sensibly elongated in that direction, having near the extremity a small luminous point, about as bright as a star of the 9th magnitude. The object was so faint as to render observations difficult, for which reason the diameter could not be measured; but Mr. Liais estimated it at 25 or 30 seconds in the larger, and 7 or 8 seconds in the smaller diameter. The second or smaller nebulosity appeared nearly circular, and about 4 seconds in diameter. On February 27th, at 10^{h} 25^{m} it followed the other by about 27 seconds, being about 1′ 8″ to the North of it. On March 3d, at 11^{h} 16^{m}, the difference of position was 23 seconds in right ascension, and 46″ in declination. On the 6th, the moonlight wholly extinguished the comet.

From observations made by Mr. Liais with the ring-micrometer, Dr. Pape, of Altona, has computed three normal places, and thence deduced elements as follows, viz.:—

Time of perihelion passage, Febr. 16·7667, Berlin m. t.		
Long. of perihelion, - -	173° 26′ 2	App. eqx. Febr. 29·6
" " asc. node, - - -	324 1 9	
Inclination, - - - - -	79 22 6	
Log. perih. dist., - - - -	0·07652	
Motion, - - - -	Direct.	

2. *Second Comet of* 1860.—On the 17th of April, 1860, a telescopic comet was discovered by Mr. George Rümker, of Hamburg. It was a faint, ill-defined nebulous spot of light. Having passed its perihelion at the close of the preceding February, it was when discovered receding from the sun. Its elements, approximately determined, are found to resemble those of the second comet of 1793.

3. *On the alleged intra-Mercurial Planet.*—According to the elements assigned by M. Leverrier to the planet which Dr. Lescarbault states that he observed passing across the sun March 26, 1859, there was reason to expect that the planet would be seen in like transit, sometime in March or April of the present year. In the hope of detecting such a transit the sun's disc was closely watched during these months by observers in three places at least in this country, and doubtless also by many observers in Europe. So far as we hear, the search has everywhere proved unsuccessful. But as during this period there were many hours in which the sun's disk was not and could not be under observation, the failure does not disprove the existence of such a planet. It is to be hoped that the search will be resumed hereafter, and as there is great uncertainty respecting the inclination of the planet, a thorough observation at any time, with a magnifying power of 100 or more will be valuable. If the spot seen on the sun, February 12, 1820, by Steinhübel and by Stark, (Mon. Not. Roy. Astr. Soc., March 9, 1860,) was this planet, the inclination must be quite small, and a transit across the sun may often occur.

Lescarbault's alleged observation of March 26, 1859, is contradicted by M. Liais, in No. 1248 of the Astronomische Nachrichten. The latter states that on that day he was observing, with a view of determining the relative intensities of the different parts of the disc, at San Domingos, in the Bay of Rio Janeiro, from $11^h\ 4^m$ to $11^h\ 20^m$, and from $12^h\ 42^m$ to $1^h\ 17^m$ S. Dom. mean solar time. Allowing three hours for difference of longitude, the ingress of the spot should have been at $1^h\ 5^m$ at St. Domingo. At $1^h\ 17^m$ the spot should accordingly have been on the sun's disc for twelve minutes and would in that time have traversed an arc of $1'\ 4$. M. Liais asserts he must have seen such a spot had it been there. He shows further that such a planet as the one supposed would be easily seen during a large part of its orbit, by the telescope, and often by the naked eye of an observer within the tropics. As the planet has not been thus seen he does not believe it to exist.

V. BOOK NOTICES.

Prof. Agassiz on the Origin of Species.

1. *Contributions to the Natural History of the United States;* by L. Agassiz.—The third volume of this work, now in the press, will appear shortly. We copy from the advance sheets the following paragraphs relating to the origin of species, which has lately attracted much attention, in consequence of the publication of Darwin's book on that subject.

Individuality and Specific Differences among Acalephs.

The morphological phenomena discussed in the preceding section naturally lead to a consideration of individuality and of the extent and importance of specific differences among the Acalephs. A few years ago the prevailing opinion among naturalists was that, while genera, families, orders, classes, and any other more or less comprehensive divisions among animals were artificial devices of science to facilitate our studies, species alone had a real existence in nature. Whether the views I have presented in the first volume of this work (p. 163), where I showed that species do not exist in any different sense from genera, families, etc., have had any thing to do with the change which seems to have been brought about upon this point among scientific men, is not for me to say; but whatever be the cause, it is certainly true that, at the present day, the number of naturalists who deny the real existence of species is greatly increased. Darwin in his recent work on the "Origin of Species," has also done much to shake the belief in the real existence of species, but the views he advocates are entirely at variance with those I have attempted to establish. For many years past I have lost no opportunity of urging the idea that while species have no material existence, they yet exist as categories of thought, in the same way as genera, families, orders, classes, and branches of the animal kingdom. Darwin's fundamental idea, on the contrary, is that species, genera, families, orders, classes, and any other kind of more or less comprehensive divisions among animals do not exist at all, and are altogether artificial, differing from one another only in degree, all having originated from a successive differentiation of a primordial organic form, undergoing successively such changes as would

at first produce a variety of species; then genera, as the difference became more extensive and deeper; then families, as the gap widened still farther between the groups, until in the end all that diversity was produced which has existed or exists now. Far from agreeing with these views, I have, on the contrary, taken the ground that all the natural divisions in the animal kingdom are primarily distinct, founded upon different categories of characters, and that all exist in the same way, that is, as categories of thought, embodied in individual living forms. I have attempted to show that branches in the animal kingdom are founded upon different plans of structure, and for that very reason have embraced from the beginning representatives between which there could be no community of origin; that classes are founded upon different modes of execution of these plans, and therefore they also embrace representatives which could have no community of origin; that orders represent the different degrees of complication in the mode of execution of each class, and therefore embrace representatives which could not have a community of origin any more than the members of different classes or branches; that families are founded upon different patterns of form, and embrace representatives equally independent in their origin; that genera are founded upon ultimate peculiarities of structure, embracing representatives, which, from the very nature of their peculiarities could have no community of origin; and that finally, species are based upon relations and proportions that exclude, as much as all the preceding distinctions, the idea of a common descent.

As the community of characters among the beings belonging to these different categories arises from the intellectual connection which shows them to be categories of thought, they cannot be the result of a gradual material differentiation of the objects themselves. The argument on which these views are founded may be summed up in the following few words: Species, genera, families, &c. exist as thoughts, individuals as facts. It is presented at full length in the first volume of this work, (p. 137–168), where I have shown that individuals alone have a definite material existence, and that they are, for the time being, the bearers not only of specific characteristics, but of all the natural features in which animal life is displayed in all its diversity; individuality being, in fact, the great mystery of organic life.

Since the arguments presented by Darwin in favor of a universal derivation from one primary form, of all the peculiarities existing now among living beings have not made the slightest impression on my mind, nor modified in any way the views I have already propounded, I may fairly refer the reader to the paragraphs alluded to above as containing sufficient evidence of their correctness, and I will here only add a single argument, which seems to leave the question where I have placed it.

It seems to me that there is much confusion of ideas in the general statement of the variability of species so often repeated lately. If species do not exist at all, as the supporters of the transmutation theory maintain, how can they vary? and if individuals alone exist, how can the differences which may be observed among them prove the variability of species? The fact seems to me to be that while species are based upon definite relations among individuals which differ in various ways among themselves,

each individual, as a distinct being, has a definite course to run from the time of its first formation to the end of its existence, during which it never loses its identity nor changes its individuality, nor its relations to other individuals belonging to the same species, but preserves all the categories of relationship which constitute specific or generic or family affinity, or any other kind or degree of affinity. *To prove that species vary it should be proved that individuals born from common ancestors change the different categories of relationship which they bore primitively to one another.* While all that has thus far been shown is, that there exists a considerable difference among individuals of one and the same species. This may be new to those who have looked upon every individual picked up at random, as affording the means of describing satisfactorily any species; but no naturalist who has studied carefully any of the species now best known, can have failed to perceive that it requires extensive series of specimens accurately to describe a species, and that the more complete such series are, the more precise appear the limits which separate species. Surely the aim of science cannot be to furnish amateur zoölogists or collectors, a *recipe* for a ready identification of any chance specimen that may fall into their hands. And the difficulties with which we may meet in attempting to characterize species do not afford the first indication that species do not exist at all, as long as most of them can be distinguished, as such, almost at first sight. I foresee that some convert to the transmutation creed will at once object that the facility with which species may be distinguished is no evidence that they were not derived from other species. It may be so. But as long as no fact is adduced to show that any one well known species among the many thousands that are buried in the whole series of fossiliferous rocks, is actually the parent of any one of the species now living, such arguments can have no weight; and thus far the supporters of the transmutation theory have failed to produce any such facts. Instead of facts we are treated with marvelous bear, cuckoo, and other stories. Credat Judaeus Apella!

Had Mr. Darwin or his followers furnished a single fact to show that individuals change, in the course of time, in such a manner as to produce at last species different from those known before, the state of the case might be different. But it stands recorded now as before, that the animals known to the ancients are still in existence, exhibiting to this day the characters they exhibited of old. The geological record, even with all its imperfections, exaggerated to distortion, tells now, what it has told from the beginning, that the supposed intermediate forms between the species of different geological periods are imaginary beings, called up merely in support of a fanciful theory. The origin of all the diversity among living beings remains a mystery as totally unexplained as if the book of Mr. Darwin had never been written, for no theory unsupported by fact, however plausible it may appear, can be admitted in science.

It seems generally admitted that the work of Darwin is particularly remarkable for the fairness with which he presents the facts adverse to his views. It may be so; but I confess that it has made a very different impression upon me. I have been more forcibly struck by his inability to perceive when the facts are fatal to his argument, than by anything else in the whole work. His chapter on the Geological Record, in par-

ticular, appears to me, from beginning to end, as a series of illogical deductions and misrepresentations of the modern results of Geology and Palæontology. I do not intend to argue here, one by one, the questions he has discussed. Such arguments end too often in special pleading, and any one familiar with the subject may readily perceive where the truth lies by confronting his assertions with the geological record itself. But since the question at issue is chiefly to be settled by palæontological evidence, and I have devoted the greater part of my life to the special study of the fossils, I wish to record my protest against his mode of treating this part of the subject. Not only does Darwin never perceive when the facts are fatal to his views, but when he has succeeded by an ingenious circumlocution in overleaping the facts, he would have us believe that he has lessened their importance or changed their meaning. He would thus have us believe that there have been periods during which all that had taken place during other periods was destroyed, and this solely to explain the absence of intermediate forms between the fossils found in successive deposits, for the origin of which he looks to those missing links; whilst every recent progress in Geology shows more and more fully how gradual and successive all the deposits have been which form the crust of our earth.—He would have us believe that entire faunæ have disappeared before those were preserved, the remains of which are found in the lowest fossiliferous strata; when we find everywhere non-fossiliferous strata below those that contain the oldest fossils now known. It is true, he explains their absence by the supposition that they were too delicate to be preserved; but any animals from which Crinoids, Brachiopods, Cephalopods, and Trilobites could arise, must have been sufficiently similar to them to have left, at least, traces of their presence in the lowest non-fossiliferous rocks, had they ever existed at all.—He would have us believe that the oldest organisms that existed were simple cells, or something like the lowest living beings now in existence; when such highly organized animals as Trilobites and Orthoceratites are among the oldest known.—He would have us believe that these lowest first-born became extinct in consequence of the gradual advantage some of their more favored descendants gained over the majority of their predecessors; when there exist now, and have existed at all periods in past history, as large a proportion of more simply organized beings, as of more favored types, and when such types as Lingula were among the lowest Silurian fossils, and are alive at the present day.—He would have us believe that each new species originated in consequence of some slight change in those that preceded; when every geological formation teems with types that did not exist before.—He would have us believe that animals and plants became gradually more and more numerous; when most species appear in myriads of individuals, in the first bed in which they are found. He would have us believe that animals disappear gradually; when they are as common in the uppermost bed in which they occur as in the lowest, or any intermediate bed. Species appear suddenly and disappear suddenly in successive strata. That is the fact proclaimed by Palæontology; they neither increase successively in number, nor do they gradually dwindle down; none of the fossil remains thus far observed show signs of a gradual improvement or of a slow decay.—He would have us believe that

geological deposits took place during the periods of subsidence; when it can be proved that the whole continent of North America is formed of beds which were deposited during a series of successive upheavals. I quote North America in preference to any other part of the world, because the evidence is so complete here that it can only be overlooked by those who may mistake subsidence for the general shrinkage of the earth's surface in consequence of the cooling of its mass. In this part of the globe, fossils are as common along the successive shores of the rising deposits of the Silurian system, as anywhere along our beaches; and and each of these successive shores extends from the Atlantic States to the foot of the Rocky Mountains. The evidence goes even further; each of these successive sets of beds of the Silurian system contains peculiar fossils, neither found in the beds above nor in the beds below, and between them there are no intermediate forms. And yet Darwin affirms that "the littoral and sub-littoral deposits are continually worn away, as soon as they are brought up by the slow and gradual rising of the land within the grinding action of the coast waves." Origin of Species, p. 290.—He would also have us believe that the most perfect organs of the body of animals are the product of gradual improvement, when eyes as perfect as those of the Trilobites are preserved with the remains of these oldest animals.—He would have us believe that it required millions of years to effect any one of these changes; when far more extraordinary transformations are daily going on, under our eyes, in the shortest periods of time, during the growth of animals.—He would have us believe that animals acquire their instincts gradually; when even those that never see their parents, perform at birth the same acts, in the same way, as their progenitors.—He would have us believe that the geographical distribution of animals is the result of accidental transfers; when most species are so narrowly confined within the limits of their natural range, that even slight changes in their external relations may cause their death. And all these, and many other calls upon our credulity, are coolly made in the face of an amount of precise information, readily accessible, which would overwhelm any one who does not place his opinions above the records of an age eminently characterized for its industry, and during which, that information was laboriously accumulated by crowds of faithful laborers.

It would be superfluous to discuss in detail the arguments by which Mr. Darwin attempts to explain the diversity among animals. Suffice it to say, that he has lost sight of the most striking of the features, and the one which pervades the whole, namely, that there runs throughout Nature unmistakable evidence of thought, corresponding to the mental operations of our own mind, and therefore intelligible to us as thinking beings, and unaccountable on any other basis than that they owe their existence to the working of intelligence; and no theory that overlooks this element can be true to nature.

There are naturalists who seem to look upon the idea of creation, that is a manifestation of an intellectual power by material means, as a kind of bigotry; forgetting, no doubt, that whenever they carry out a thought of their own, they do something akin to creating, unless they look upon their own elucubrations as something in which their individuality is not concerned, but arising without an intervention of their mind, in conse-

quence of the working of some "bundles of forces," about which they know nothing themselves. And yet such men are ready to admit that matter is omnipotent, and consider a disbelief in the omnipotence of matter as tantamout to imbecility; for, what is the assumed power of matter to produce all finite beings, but omnipotence? And what is the outcry raised against those who cannot admit it, but an insinuation that they are *non-compos?* The book of Mr. Darwin is free of all such uncharitable sentiments towards his fellow-laborers in the field of science; nevertheless his mistake lies in a similar assumption that the most complicated system of combined thoughts can be the result of accidental causes; for he ought to know, as every physicist will concede, that all the influences to which he would ascribe the origin of species are accidental in their very nature, and he must know, as every naturalist familiar with the modern progress of science does know, that the organized beings which live now, and have lived in former geological periods, constitute an organic whole, intelligibly and methodically combined in all its parts. As a zoölogist he must know in particular, that the animal kingdom is built upon four different plans of structure, and that the reproduction and growth of animals takes place according to four different modes of development, and that unless it is shown that these four plans of structure, and these four modes of development, are transmutable one into the other, no transmutation theory can account for the origin of species. The fallacy of Mr. Darwin's theory of the origin of species by means of natural selection, may be traced in the first few pages of his book, where he overlooks the difference between the voluntary and deliberate acts of selection applied methodically by man to the breeding of domesticated animals and the growing of cultivated plants, and the chance influences which may effect animals and plants in the state of nature. To call these influences "natural selection," is a misnomer which will not alter the conditions under which they may produce the desired results. Selection implies design; the powers to which Darwin refers the order of species, can design nothing. Selection is no doubt the essential principle on which the raising of breeds is founded, and the subject of breeds is presented in its true light by Mr. Darwin; but this process of raising breeds by the selection of favorable subjects, is in no way similar to that which regulates specific differences. Nothing is more remote from the truth than the attempted parallelism between the breeds of domesticated animals and the species of wild ones. Did there exist such a parallelism, as Darwin maintains, the difference among the domesticated breeds should be akin to the differences among wild species, and afford a clue to determine their relative degree of affinity by a comparison with the pedigrees of well-known domesticated races. Again, if there were any such parallelism, the distinctive characteristics of different breeds should be akin to the differences which exist between fossil species of earlier periods and those of the same genera now living. Now let any one familiar with the fossil species of the genera Bos and Canis, compare them with the races of our cattle and of our dogs, and he will find no correspondence whatever between them; for the simple reason that they do not owe their existence to the same causes. It must therefore be distinctly stated that Mr. Darwin has failed to establish a connection between the mode of raising domesticated

breeds, and the cause or causes to which wild animals owe their specific differences.

It is true, Mr. Darwin states that the close affinity existing among animals can only be explained by a community of descent, and he goes so far as to represent these affinities as evidence of such a genealogical relationship; but I apprehend that the meaning of the words he uses has misled him into the belief that he had found the clue to phenomena which he does not even seem correctly to understand. There is nothing parallel between the relations of animals belonging to the same genus or the same family, and the relations between the progeny of common ancestors. In the one case we have the result of a physiological law regulating reproduction, and in other affinities which no observation has thus far shown to be in any way connected with reproduction. The most closely allied species of the same genus or the different species of closely allied genera, or the different genera of one and the same natural family, embrace representatives which at some period or other of their growth resemble one another more closely than the nearest blood relations; and yet we know that they are only stages of development of different species distinct from one another at every period of their life. The embryo of our common fresh water turtle, *Chrysemis picta*, and the embryo of our snapping turtle, *Chelydra serpentina*, resemble one another far more than the different species of Chrysemis in their adult state, and yet not a single fact can be adduced to show that any one egg of an animal has ever produced an individual of any species but its own. A young snake resembles a young turtle or a young bird much more than any two species of snakes resemble one another; and yet they go on reproducing their kinds, and nothing but their kinds. So that no degree of affinity, however close, can, in the present state of our science, be urged as exhibiting any evidence of community of descent, while the power that imparted all their peculiarities to the primitive eggs of all the species now living side by side, could also impart similar peculiarities with similar relations, and all degrees of relationship, to any number of other species that have existed. Until, therefore it can be shown that any one species has the ability to delegate such specified peculiarities and relations to any other species or set of species, it is not logical to assume that such a power is inherent in any animal, or that it constitutes part of its nature.* We must look to the original power that imparted life to the first being for the origin of all other beings, however mysterious and inaccessible

* The difficulty of ascertaining the natural limits of some species, and the mistakes made by naturalists when describing individual peculiarities as specific, has nothing to do with the question of the origin of species, and yet Darwin places great weight, in support of his theory, upon the differences which exist among naturalists in their views of species. Some of the metals are difficult to distinguish, and have frequently been mistaken, and the specific differences of some may be questioned; but what could that have to do with the question of the origin of metals, in the minds of those who may doubt the original difference of metals? Nothing more than the blunders of some naturalists in identifying species with the origin of species of animals and plants. The great mischief in our science now lies in the self-complacent confidence with which certain zoölogists look upon a few insignificant lines, called diagnoses, which they have the presumption to offer as characteristics of species, or, what is still worse, as checks upon others to secure to themselves a nominal priority. Such a treatment of scientific subjects is unworthy of our age.

the modes by which all this diversity has been produced may remain for us. The production of a plausible explanation is no explanation at all, if it does not cover the whole ground.

All attempts to explain the origin of species may be brought under two categories: viz. 1st, some naturalists admitting that all organized beings are created, that is to say, endowed from the beginning of their existence with all their characteristics, while 2d, others assume that they arise spontaneously. This classification of the different theories of the origin of species, may appear objectionable to the supporters of the transmutation theory; but I can perceive no essential difference between their views and the old idea that animals may have arisen spontaneously. They differ only in the modes by which the spontaneous appearance is assumed to be effected; some believe that physical agents may so influence organized beings as to modify them—this is the view of DeMaillet and the Vestiges of Creation; others believe that the organized beings themselves change in consequence of their own acts, by changing their mode of life, etc., this is the view of Lamarck; others still assume that animals and plants tend necessarily to improve, in consequence of the struggle for life, in which the favored races are supposed to survive; this is the view lately propounded by Darwin. I believe these theories will, in the end, all share the fate of the theory of spontaneous generations so called, as the facts of nature shall be confronted more closely with the theoretical assumptions. The theories of DeMaillet, Oken, and Lamarck are already abandoned by those who have adopted the transmutation theory of Darwin; and unless Darwin and his followers succeed in showing that the struggle for life tends to something beyond favoring the existence of certain individuals over that of other individuals, they will soon find that they are following a shadow. The assertion of Darwin, which has crept into the title of his work, is, that favored *races* are preserved, while all his facts go only to substantiate the assertion, that favored *individuals* have a better chance in the struggle for life than others. But who has ever overlooked the fact that myriads of individuals of every species constantly die before coming to maturity? What ought to be shown, if the transmutation theory is to stand, is that these favored individuals diverge from their specific type, and neither Darwin nor any body else has furnished a single fact to show that they go on diverging. The criterion of a true theory consists in the facility with which it accounts for facts accumulated in the course of long-continued investigations and for which the existing theories afforded no explanation. It can certainly not be said that Darwin's theory will stand by that test. It would be easy to invent other theories that might account for the diversity of species quite as well, if not better than Darwin's preservation of favored races. The difficulty would only be to prove that they agree with the facts of Nature. It might be assumed, for instance, that any one primary being contained the possibilities of all those that have followed, in the same manner as the egg of any animal possesses all the elements of the full-grown individual; but this would only remove the difficulty one step further back. It would tell us nothing about the nature of the operation by which the change is introduced. Since the knowledge we now have, that similar

metamorphoses go on in the eggs of all living beings has not yet put us on the track of the forces by which the changes they undergo are brought about, it is not likely that by mere guesses we shall arrive at any satisfactory explanation of the very origin of these beings themselves.

Whatever views are correct concerning the origin of species, one thing is certain, that as long as they exist they continue to produce generation after generation, individuals which differ from one another only in such peculiarities as relate to their individuality. The great defect in Darwin's treatment of the subject of species lies in the total absence of any statement respecting the features that constitute individuality. Surely, if individuals may vary within the limits assumed by Darwin, he was bound first to show that individuality does not consist of a sum of hereditary characteristics, combined with variable elements, not necessarily transmitted in their integrity, but only of variable elements. That the latter is not the case, stands recorded in every accurate monograph of all the types of the animal kingdom upon which minute embryological investigations have been made. It is known, that every individual egg undergoes a series of definite changes before it reaches its mature condition; that every germ formed in the egg passes through a series of metamorphoses before it assumes the structural features of the adult; that in this development the differences of sex may very early become distinct; and that all this is accomplished in a comparatively very short time, extremely short, indeed, in comparison to the immeasurable periods required by Darwin's theory to produce any change among species; and yet all this takes place without any deviation from the original type of the species, though under circumstances which would seem most unfavorable to the maintenance of the type. Whatever minor differences may exist between the products of this succession of generations are all *individual peculiarities*, in no way connected with the essential features of the species, and therefore as transient as the individuals; while the specific characters are forever fixed. A single example will prove this. All the robins of North America now living have been for a short time in existence; not one of them was alive a century ago, when Linnæus for the first time made known that species under the name of Turdus migratorius, and not one of the specimens observed by Linnæus and his cotemporaries was alive when the Pilgrims of the Mayflower first set foot upon the Rock of Plymouth. Where was the species at these different periods, and where is it now? Certainly nowhere but in the individuals alive for the time being; but not in any single one of them, for that one must be either a male or a female, and not the species; not in a pair of them, for the species exhibits its peculiarities in its mode of breeding, in its nest, in its eggs, in its young, as much as in the appearance of the adult; not in all the individuals of any particular district, for the geographical distribution of a species over its whole area, forms also part of its specific characters.* A species is only known when its whole history

* We are so much accustomed to see animals reproducing themselves, generation after generation, that the fact no longer attracts our attention, and the mystery involved in it no longer excites our admiration. But there is certainly no more marvellous law in all nature than that which regulates this regular succession. And upon this law the maintenance of species depends; for observation teaches us that all that is not individual peculiarity is unceasingly and integrally reproduced while all that constitutes individuality, as such, constantly disappears.

has been ascertained, and that history is recorded in the life of individuals through successive generations. The same kind of argument might be adduced from every existing species, and with still greater force by a reference to those species already known to the ancients.

Let it not be objected that the individuals of successive generations have presented marked differences among themselves; for these differences, with all the monstrosities that may have occurred, during these countless generations, have passed away with the individuals, as individual peculiarities, and the specific characteristics alone have been preserved, together with all that distinguishes the genus, the family, the order, the class, and the branch to which the individual belonged. Moreover all this has been maintained through a succession of repeated changes, amounting in each individual to the whole range of transformations, through which an individual passes, from the time it is individualized as an egg, to the time it is itself capable of reproducing its kind, and, perhaps, with all the intervening phases of an unequal production of males and females, of sterile individuals, of dwarfs, of giants, etc., etc., during which there were millions of chances for a deviation from the type. Does this not prove that while individuals are perishable, they transmit, generation after generation, all that is specific or generic, or, in one word, *typical* in them, to the exclusion of every *individual peculiarity* which passes away with them, and that, therefore, while individuals alone have a material existence, species, genera, families, orders, classes, and branches of the animal kingdom exist only as categories of thought in the Supreme Intelligence, but as such have as truly an independent existence and are as unvarying as thought itself after it has once been expressed.

Returning, after this digression, to the question of individuality among Acalephs, we meet here phenomena far more complicated than among higher animals. Individuality, as far as it depends upon material isolation, is complete and absolute in all the higher animals, and there maintained by genetic transmission, generation after generation. Individuality, in that sense, exists only in comparatively few of the Radiates. Among Acalephs it is ascertained only for the Ctenophoræ and some Discophoræ. In others, the individuals born from eggs end by dividing into a number of distinct individuals. In others still, the successive individuals derived from a primary one, remain connected to form compound communities. We must thererefore, distinguish different kinds and different degrees of individuality, and may call *hereditary individuality* that kind of independent existence manifested in the successive evolutions of a single egg, producing a single individual, as is observed in all the higher animals. We may call *derivative* or *consecutive individuality* that kind of independence resulting from an individualization of parts of the product of a single egg. We have derivative individuals among the Nudibranchiate Mollusks, whose eggs produce singly, by a process of complete segmentation, several independent individuals. We observe a similar phenomenon among those Acalephs the young of which (Scyphostoma) ends in producing, by transverse division (Strobila), a number of independent free Medusæ (Ephyræ). We have it also among the Hydroids which produce free Medusæ. Next, we must distinguish *secondary individuality*, which is inherent to those individuals arising as

buds from other individuals, and remaining connected with them. This condition prevails in all the immovable Polyparia and Hydraria, and I say intentionally in the immovable ones; for, in the movable communities, such as Renilla, Pennatula, etc., among Polyps, and all the Siphonophoræ among Acalephs, we must still further distinguish another kind of individuality, which I know not how to call properly, unless the name of *complex individuality* may be applied to it. In complex individuality a new element is introduced, that is not noticeable in the former case. The individuals of the community are not only connected together, but, under given circumstances, they act together as if they were one individual, while at the same time each individual may perform acts of its own.

As to the specific differences observed among Acalephs, there is as great a diversity between them as between their individuals. In some types of this class the species are very uniform; all the individuals belonging to one and the same species resembling one another very closely, and exhibiting hardly any difference among themselves, except such as arises from age. This identity of the individuals of one and the same species is particularly striking among the Ctenophoræ. In this order there are not even sexual differences among the individuals, as they are all hermaphrodites. In the Discophoræ proper a somewhat greater diversity prevails. In the first place we notice male and female individuals, and the difference between the sexes is quite striking in some genera, as, for instance, in Aurelia. Next there occur frequent deviations, among them, in the normal number of their parts; their body consisting frequently of one or two spheromeres more than usual, sometimes, even, of double normal number, or of a few less. And yet, year after year, the same Discophoræ reappear upon our shores, with the same range of differences among their individuals. Among Hydroids polymorphism prevails to a greater or less extent, besides the differences arising from sex. Few species have only one kind of individuals. Mostly the cycle of individual differences embraces two distinct types of individuals, one recalling the peculiarities of common Hydræ, the other those of Medusæ; but even the Hydra type of one and the same species may exhibit more or less diversity, there being frequently two kinds of Hydræ united in one and the same community, and sometimes even a larger number of heterogeneous Hydræ. And this is equally true, though to a less extent, of the Medusa type. Yet among Siphonophoræ there are generally at least two kinds of Medusæ in one and the same community. But notwithstanding this polymorphism among the individuals of one and the same community, genetically connected together, each successive generation reproduces the same kinds of heterogeneous individuals, and nothing but individuals linked together in the same way. Surely we have here a much greater diversity of individuals, born one from the other, than is exhibited by the most diversified breeds of our domesticated animals; and yet all these heterogeneous individuals remain true to their species, in once case as in the other, and do not afford the slightest evidence of a transmutation of species.

Would the supporters of the fanciful theories lately propounded, only extend their studies a little beyond the range of domesticated animals, would they investigate the alternate generations of the Acalephs,

the extraordinary modes of development of the Helminth, the reproduction of the Salpæ, etc., etc., they would soon learn that there are, in the world, far more astonishing phenomena, strictly circumscribed between the natural limits of unvarying species, than the slight differences produced by the intervention of men, among domesticated animals, and, perhaps, cease to be so confident as they seem to be, that these differences are trustworthy indications of the variability of species. For my own part I must emphatically declare that I do not know a single fact tending to show that species do vary in any way, while it is true that the individuals of one and the same species are more or less polymorphous. The circumstance, that naturalists may find it dificult to trace the natural limits of any one particular species, or the mistakes they may make in their attempts to distinguish them, has nothing whatsoever to do with the question of their origin.

There is another feature of the species of Acalephs which deserves particularly to be noticed. All these animals are periodical in their appearance, and last for a short period, in their perfect state of development. In our latitude most Medusæ make their appearance, as Ephyræ, early in the Spring, and rapidly enlarge to their full size. In September and October they lay their eggs and disappear; the young hatched from the eggs move about, as Planulæ, for a short time, and then become attached, as Scyphostomes, and pass the winter in undergoing their Strobila metamorphosis. The Ctenophoræ appear also very early, and lay their eggs in the autumn, passing the winter as young, and growing to their full size towards the beginning of the summer. Among the Hydroids there is more diversity in their periodicity. Hydraria are found all the year round; but the Medusæ buds, the free Medusæ, and the Medusaria make their appearance in different seasons, in different species. Some bring forth Medusæ buds and free Medusæ or Medusaria during winter; others, and in our latitude this is the case with by far the largest number of the Hydroids, produce their Medusæ brood in the spring; a few breed later, in the summer or in the autumn; so that, notwithstanding the regularity of their periodical return, Acalephs may be studied, in some condition or other, during the whole year.

When considering Individuality and Specific Differences, as manifested in the class of Acalephs, I have taken an opportunity of showing, upon general grounds how futile the arguments are upon which the theory of transmutation of species is founded. Having now shown that that class is circumscribed within definite limits, I may be permitted to add here a few more objections to that theory, based chiefly upon special grounds, connected with the characteristics of classes. If there is any thing striking in the features which distinguish classes, it is the definiteness of their structural peculiarities; and this definiteness goes on increasing, with new and additional qualifications, as we pass from the class characters to those which mark the orders, the families, the genera, and the species. Granting, for the sake of argument, that organized beings living at a later period may have originated by a gradual change of those of earlier periods, one of the most characteristic features of all organized beings remains totally unexplained by the various

theories brought forward to explain that change; the definiteness of their respective groups, be they ever so comprehensive, or ever so limited, combined with the greatest inequality in their numeric relations. There exist a few thousand Mammalia and Reptiles, and at least three times their number of Birds and Fishes. There may be twenty thousand Mollusks; but there are over a hundred thousand Insects, and only a few thousand Radiates. And yet the limits of the class of Insects are as well defined as those of any other class, with the only exception of the class of Birds which is unquestionably the most definite in its natural boundaries. Now the supporters of the transmutation theory may shape their views in whatever way they please to suit the requirements of the theory, instead of building the theory upon the facts of Nature, they never can make it appear that the definiteness of the characters of the class of Birds is the result of a common descent of all Birds, for the first Bird must have been brother or cousin to some other animal that was not a Bird, since there are other animals besides Birds in this world, to no one of which any bird bears as close a relation as it bears to its own class. The same argument applies to every other class; and as to the facts, they are fatal to such an assumption, for Geology teaches us that among the oldest inhabitants of our globe known, there are representatives of nine distinct classes of animals, which by no possibility can be descendants of one another, since they are cotemporaries.

The same line of argument and the same class of facts forbid the assumption that either the representatives of one and the same order, or those of one of the same family, or those of one of the same genus should be considered as lineal descendants of a common stock; for orders, families and genera are based upon different categories of characters, and not upon more or less extensive characters of the same kind, as I have shown years ago (Vol. I, p. 150 to 163), and numbers of different kinds of representatives of these various groups, make their appearance simultaneously in all the successive geological periods. There appear together Corals and Echinoderms of different families and of different genera in each successive geological formation, and this is equally true for Bryozoa, Brachiopods and Lamellibranchiata, for Trilobites and the other Crustacea, in fact for the representatives of all the classes of the animal kingdom, making due allowance for the period of the first appearance of each; and at all times and in all classes the representatives of these different kinds of groups are found to present the same definiteness in their characteristics and limitation. Were the transmutation theory true, the geological record should exhibit an uninterrupted succession of types blending gradually into one another. The fact is that throughout all geological times each period is characterized by definite specific types, belonging to definite genera, and these to definite families, referable to definite orders, constituting definite classes and definite branches, built upon definite plans. Until the facts of Nature are shown to have been mistaken by those who have collected them, and that they have a different meaning from that now generally assigned to them, I shall therefore consider the transmutation theory as a scientific mistake, untrue in its facts, unscientific in its method, and mischievous in its tendency.

Cambridge, June 30, 1860.

2. *Anleitung zur Organischen und Gasanalyse von* J. SCHIEL. Erlangen, 1860. 260 pp., 8vo.—This introduction to elementary organic and gas analysis, by our adopted countryman Dr. Schiel of St. Louis, contains a very full, yet concise, account of all of the approved methods, and is an excellent hand-book of these subjects. Many of the refinements of research which the text-books do not notice, and newly discovered processes or apparatus that are beginning to displace those hitherto in vogue, we find satisfactorily described in these pages. The best and simplest methods for taking the specific gravity of liquids, for determining melting and boiling points are given, and the admirable process of Simpson for the quantitative determination of nitrogen as well as that of Natanson for taking vapor densities are fully described.

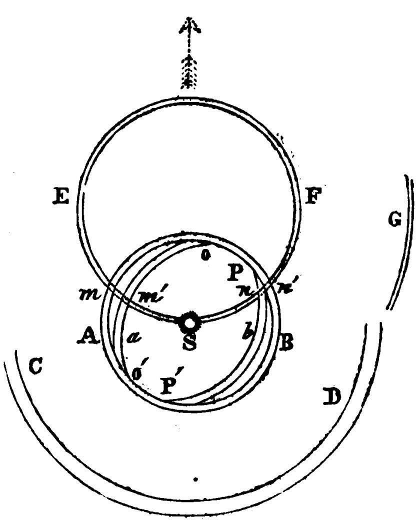
The chapters on Gas Analysis present in a clear manner the important parts of this subject. An instructive section of the book is occupied with an exposition of the mathematical principles which apply to the discussion of experimental results and the correction of errors of observation.

We believe that this treatise will be of great service in the hands of students, and by its careful selection and full description of the methods that unite simplicity and accuracy, will help to inspire them with the *animus* of scientific research.

VI. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Parhelia seen at Weld, Franklin Co., Me.;* by STILLMAN MASTERMAN, (in a letter to the Editors).—*Messrs. Editors:*—I send you the following account of a *parhelion* seen by myself, at this place on the 21st instant, at $10^h\,45^m$ A. M.

A light haze was spread over the sky at the time, for the most part so thin as to be scarcely visible, but at a few points coming out in lenticular patches of well-marked *cirrus.* When first noticed at the time above stated, the meteor presented the appearance delineated in the accompanying drawing. The circle A B was about 45° in diameter, having the sun in its centre; and was very brilliant, having the colors of the primary rainbow, but with the red next to the sun. It was surrounded by a bright corona of white, three or four degrees in width: the width of the colored ring A B being equal to the apparent diameter of the solar orb. Two elliptical arcs *a* and *b* were included in A B, having their middles 2° distant from the inner edge of the latter and their extremities falling within it at *o o′* and P P′. In color and width *a* and *b* were precisely like A B and of a like brightness.



Concentric with A B was the arc C D, being a little less than a semicircle, on a diameter of 95°, colored like A B with its red next to the

sun, but of twice the width of the latter, and of dazzling brilliancy. The circle E F, with a diameter of about 60°, had its circumference in the sun and its centre vertically above him, [the arrow denoting the position of the vertical passing through the sun,] and consisted of a ring of pure white, quite brilliant, and having a width equal to two-thirds the apparent diameter of the sun. The arc G was concentric with E F, and bore the same proportion in radius to the latter that C D did to A B. It was white like E F, but not nearly so bright. The *parhelia* at $m\ m'$ and $n\ n'$ were equal to the united brilliancy of the rings intersecting at those points.

At $11^h\ 10^m$, all the rings had vanished but A B, which had contracted in diameter and width, and a small portion of C D immediately below the sun.

At $11^h\ 30^m$, A B had regained its former dimensions, with increased brilliancy. C D had entirely vanished.

At $12^h\ 0^m$, A B had contracted to 30° diameter, and become faint. All traces of the meteor had entirely vanished at $1^h\ 30^m$ P. M. The *cirrus* had in the mean time become gradually converted into *cirro-cumulus*, and the thin haze had completely vanished.

Weld, Franklin Co., Maine, May 28th, 1860.

2. *Tolles' improved Microscope Objectives.*—It has been generally supposed that the further improvement of object-glasses for the microscope was to be sought in the production of flint glass of high dispersive power. I am happy to inform your readers that Mr. R. B. Tolles of Canastota, N. Y., has boldly discarded the use of flint glass of unusual dispersive power and has discovered a method of constructing glasses of as large aperture as have ever yet been made by any artist, using only such glass of uniform density as is made for ordinary optical purposes.

I have recently received from Mr. Tolles a *one-third inch objective*, constructed on his plan, which has an aperture of 100° and is remarkable for the great amount of light transmitted even at the extreme borders of the lens. This objective is well corrected for chromatic and spherical aberration and gives excellent definition of blood corpuscles as well as of other tests appropriate for such an objective. This is a remarkably fine glass for ordinary investigations in minute anatomy and pathology. Mr. Tolles makes a one-fourth inch objective of 140° to 160° aperture, and a one-eighth inch of 160° to 175° aperture. I am inclined to think Mr. Tolles' glasses are unsurpassed by any object-glasses for the microscope that have yet been made in this country or elsewhere. M. C. White.

New Haven, June 10, 1860.

3. *Application of Photography in construction of Micrometers.*—The successful application of photography in the construction of micrometers, has been made by Mr. Clarence Morfit of the U. S. Assay Office, New York. It is merely the reduction of a large scale of exact dimensions and divisions to a definite size suitable for microscopic instruments. A scale of ten inches divided into inches and tenths of an inch has been reduced in this manner to one-twentieth of an inch, thus making its smallest divisions equal to one two-thousandth part of an inch square. The method is simple, accurate and economical. Moreover, the micrometer has the advantage of giving the exact measurement of the object in frac-

tions of an inch and at the same time determines the power of the microscope itself.

4. *Geological Survey of California.*—The Act of the California legislature (approved April 24, 1860) appoints Prof. J. D. Whitney State Geologist with a salary of $6000 per annum. He appoints his own assistants with the approval of the Governor. Twenty thousand dollars are appropriated to the payment of expenses incurred in the survey. The provisions of the Act respecting the annual and final Reports are eminently judicious, especially that part which directs the Governor and Secretary of State to cause the volumes of the geological survey to be sold for the benefit of the Common School fund of the State.

No state geological survey was ever more auspiciously inaugurated, wisely provided for, or fraught with more interesting scientific and practical problems. It is understood that Prof. Whitney will commence his Californian labors in the coming autumn.

5. *Total Solar Eclipse of July* 18, 1860.—The American Nautical Almanac Commission has sent three observers to the Cumberland House, British America, to take note of this eclipse. This is a station of the Hudson's Bay Company on the West side of Pine Island Lake, in lat. 54° N., long. 102° 40′ W.

Another party of observers, consisting of Prof. Stephen Alexander, of Princeton, N. J., President F. A. P. Barnard, of Oxford, Mississippi, Prof. C. S. Venable, of North Carolina, Prof. A. W. Smith of the Naval Academy, Annapolis, Md., and Lieut. E. D. Ashe, R. N., of the Quebec Observatory, embarked June 28th at New York in the U. S. steamer Bibb, Lt. Commanding Alex. Murray, to proceed to Cape Chadleigh, Labrador.

6. *Newport Meeting of the American Association for the Advancement of Science.*—The next meeting of the Association will be held at Newport, R. I., commencing on Wednesday the first of August. The officers of the meeting are: President, Isaac Lea, Esq. of Philadelphia; Vice President, Dr. B. A. Gould of Cambridge; General Secretary, Prof. Joseph LeConte of Columbia, S. Ca.; Treasurer, Dr. A. L. Elwyn of Philadelphia.

It will be remembered that Prof. Henry, at the request of the Association, will deliver a discourse commemorative of the life and scientific labors of Dr. ROBERT HARE. Prof. Bache will also by appointment give an address on the Gulf Stream; while Prof. Leidy was requested to prepare a discourse on the extinct Reptilia and Mammalia of North America.

There are many reasons why this may be looked forward to as one of the most attractive and promising meetings of the Association ever held.

7. *Letter from John McCrady, Esq., Charleston, on the* Lingula pyramidata *described by Mr. W. Stimpson*, vol. xxix, p. 444.—DEAR STIMPSON: I believe you are connected with the Zoological department of Silliman's Journal during the absence of Prof. Dana. You have also, I see, redescribed there our Lingula, long known to naturalists but not before described. It was found more than ten years ago on our coast by the Rev. Thomas J. Young. A living specimen was also found by Mr. Burkhardt on Sullivan's Island when Prof. Agassiz had a laboratory there. I have never seen a specimen during all the time I have resided on Sullivan's Island. I am very glad you have distinguished and described it, since I suppose the Beaufort species can hardly be different from the South Carolinian.

I have found quite lately something very interesting in connection with this *Lingula pyramidata.* You know that up to this time nothing has been known to science of the development of the Brachiopoda. I have within a few days discovered an embryo unlike anything known, and which has at once so many affinities with the Bryozoa and the Brachiopoda, that I believe it to be an embryo Brachiopod, and very probably the young of *Lingula pyramidata.* Imagine an equilateral, thin, hyaline, straight-hinged bivalve shell, elliptical in outline, and with valves very flat. Through this perfectly transparent shell is visible a lining membrane or rather the borders of such a membrane, which is the mantle. Within this and near the hinge, a large flask-shaped body containing a digestive cavity surrounded by a dark mass. This cavity extends into the neck of the bottle shaped cavity (œsophagus) and terminates towards the gap of the shell in a mouth. From the opposite or basal end of the digestive cavity goes off a pretty long intestine which turns first to the left, makes several convolutions and terminates in an anus on the right side between the two valves of the shell. The mouth lies on a somewhat triangular prolongation of the body-wall which rests with its apex towards the gap of the shell in the dorsal valve. The borders right and left of this homologue of the arms are fringed each about six cirrhi, the hindermost being the longest.

The animal thus constituted, when quiet withdraws its whole body, cirrhi and all, within its bivalve shell, which is tightly closed; but when in motion the shell is distinctly opened and the gap of the valves is plainly visible even to the naked eye. Through their aperture are thrust out the cirrhi, about twelve in number, which then arrange themselves in circular funnel-like manner precisely as in a Bryozoan polyp, and by a motion plainly ciliary, and with its cirrhi or tentacula thus extended the embryo swims through the water with considerable rapidity.

The cirrhi of this embryo I take to be the homologues of the cirrhi of Brachiopoda, and of Cristatella, and of the tentacula of Bryozoa generally. The rest of the structure, especially the anus on the right and the shape of the shell, point, I think, to Lingula embryo with cirrhi extended about a line in length. There is no trace of a peduncle. It appears to me that this must set at rest all difficulty about the approximation of Bryozoa and Brachiopoda, as proposed by Agassiz and others. A somewhat fuller description of this young animal, with a figure, will be published in the forthcoming issues of the Elliott Society of Charleston.

Charleston, S. C., June 18th, 1860.

8. *The Fusion and Casting of Platinum* on a considerable scale has been accomplished by Messrs. Deville and Debray. At the sitting of the *Académie des Sciences* at Paris, June 4, they exhibited (1.) Two ingots of platinum weighing together 25 kilograms, fused in the same fire and cast in an ingot mould of cast iron. The surface of the metal shows evidence of perfect fluidity and carries the impression of characters engraved on the surface of the mould.

(2.) A toothed wheel, of platinum cast in ordinary founders' sand was also shown. This was cast in the mode common for cast iron in a two-part flask with a sprue and vent holes as usual.

The metal used they obtained by the dry mode from the crude platinum and platinum-money which the Russian Government had placed at their disposal; the details of the process being reserved for a second communication to the Academy.—*Comptes Rendus*, June 14, l, 138.

9. *New Arctic Expedition by* Dr. I. I. HAYES.—Dr. Hayes, well known as the companion of Kane and the author of "An Arctic Boat Voyage," leaves Boston in a small vessel with seventeen companions for Smith's Sound, early in July, designing to test the views respecting the open sea which he has already expressed in detail in this Journal in an article entitled "Observations on the practicability of reaching the North Pole."* This new expedition of Dr. Hayes is equipped by private liberality, and goes out under the auspices of the American Geographical Society.

In this connexion we may mention the proposal of Mr. PARKER SNOW of London, well known for his Arctic researches, to set on foot a new exploration to search for further traces of the Franklin Expedition. Mr. Snow has prepared a paper setting forth his plans and motives in detail, a copy of which we have received and placed before the American Geographical Society at a late sitting, by whom it has been officially communicated to Dr. Hayes, just on the eve of his departure.

10. *Constitution and By-Laws of and List of Officers and Members of the Chicago Academy of Sciences.* Chicago, Illinois, 1859.—This flourishing young association of naturalists was organized in 1857, and incorporated March, 1859. Its establishment affords another gratifying proof of the rapid progress of science at the West.

11. *Personal.*—Prof. A. D. BACHE, Superintendent of the U. S. Coast Survey, was recently chosen a Foreign Member of the Royal Society of London. Of the fifty foreign members, three are now Americans, viz., Professors Peirce, Agassiz, and Bache.

Prof. DANA has been in Switzerland since early in May, and will remain among the mountains until his return, toward the close of summer. His health is slowly but steadily improving.

Prof. BENJAMIN PEIRCE, of Cambridge, has sought relief from his too severe labors in a voyage to Europe by packet. His numerous friends will be pained to learn that his health was much impaired.

Prof. J. P. COOKE, of Cambridge, lately read before the Chemical Society of London a paper on the compounds of antimony and zinc ($SbZn^2$ $SbZn^3$), designed to show that crystalline form is not a necessary indication of definite chemical composition. A copy of this paper from the Author reached us at too late a date for publication in the present number of this Journal.

The gold medals of the London Geographical Society have been awarded (May 28), at the suggestion of Sir R. I. Murchison, to LADY FRANKLIN and SIR LEOPOLD MCCLINTOCK;—to the first in consideration of the deeds of her husband and in recognition of her noble-minded devotion; and to the second for his discovery of the fate of the Erebus and Terror and the accompanying additions to Arctic geography.

LADY FRANKLIN is announced as expected soon to arrive in the United States as the guest of Henry Grinnell, Esq., Vice-President of the American Geographical Society.

* Vol. xxvi, No. 78, 305, Nov. 1858.

12. *To our Correspondents* we owe an apology for the unavoidable postponement to a succeeding Number of several valuable papers, crowded out by the unexpected length of the Articles in the present issue. Even the addition of a full sheet has not sufficed to embrace our customary lists of new books, or a considerable mass of scientific intelligence, proceedings of societies, bibliography, &c., already in type.

13. *Obituary.*—The Rev. BADEN POWELL, Professor of Geometry in the University of Oxford and equally distinguished as a mathematician and physicist, died there in June.

CHARLES GOODYEAR, widely known as the discoverer of "*vulcanization*" of Caoutchouc died in New York, July 1st, aged fifty-nine years.

CANADIAN NATURALIST AND GEOLOGIST, Dec., 1859. Vol. IV.—p. 411, Notes on land and sea birds observed around Quebec; *J. M. LeMoine.*—p. 414, Chemical Geology; *Hunt.*—p. 426, Fossils of the Chazy limestone, with new species; *Billings.* —p. 494, A list of the Coleoptera found in the vicinity of Montreal; *D'Urban.*—Feb. 1860, Vol. V.—p. 1, Devonian plants of Canada; *Dawson.*—p. 14, List of plants found near Prescott, C. W.; *Billings.*—p. 24, Tubicolous Marine Worms of the Gulf of St. Lawrence; *Dawson.*—p. 30, Marine Algæ; *Kemp.*—p. 69, Palæozoic fossils; *Billings.* April.—p. 81, Natural History of the Valley of the River Rouge; *D'Urban.*—p. 100, Review of Darwin; *Dawson.*—p. 120, Sketch of the life of Mr. David Douglas.—p. 132, On the Silurian and Devonian rocks of Nova Scotia; *Dawson.*—p. 144, New fossils from the Silurian of Nova Scotia.

PROCEEDINGS PHILADELPHIA ACAD. NAT. SCI., 1860.—p. 81, Notice of the death of Dr. Hallowell.—p. 82, Habits of an Ocelot kept on ship-board; *Wilson.*—p. 85, Corrected numbers of Unionidæ; *Lea.*—On Hyalonema mirabilis from Japan; *Leidy.* —p. 86, Donation of 28,000 specimens of birds, by Dr. T. B. Wilson.—p. 88, On the coloring matter of the Nacre in Uniones; *Lea.*—p. 89, New South American Unionidæ; *Lea.*—p. 92, New Uniones and Melaniæ; *Lea.*—p. 93, New Cretaceous fossils from New Jersey; *Gabb.*—p. 96, Experiments in chemical geology; *Lesley.* —On Trichina spiralis; *Leidy.*—p. 97, On the palpi of S. American Anodons; *Lea.* —On a gneiss boulder in Orange Co., N. Y.; *Lesley.*—p. 98, New botanical locality; *Leidy.*—On the Albertite of New Brunswick; *Rogers.*—Experiments in Binocular Vision; *Rogers.*—p. 100, Conspectus piscium, etc., the Sicydianæ collected on the North Pacific Expedition; *Theo. Gill.*—p. 102, Monograph of the genus Labrosomus, Sw.; *Gill.*—p. 108, Monograph of the genus Labrax, Cuv.; *Gill.* (Mr. Gill finds our American species generically distinct from the European type. Our striped basse he places in the genus *Roccus*, and the white perch in *Morone*—names adopted from Mitchell.)—p. 120, Monograph of the Philypni; *Gill.*—p. 126, Notice of geological discoveries made by Capt. J. H. Simpson; *Meek* and *Engelman.*—p. 132, Catalogue of Birds collected on the Isthmus of Darien by the expedition under Lt. Michler, with notes and descriptions of new species; *Cassin.*

JOURNAL OF THE ACADEMY OF NATURAL SCIENCES OF PHILADELPHIA, New Series, Vol. IV, Pt. III, March, 1860.—Descriptions of Edotic Unionidæ, 13 plates; *I. Lea.* —New Cretaceous and Eocene fossils from Mississippi and Alabama (2 plates); *T. A. Conrad.*—New Cretaceous and Triassic fossils (a plate); *Gabb.*—Reflections upon the nature of the temporary star of the year 1572,—an application of the nebular hypothesis; *Wilcocks.* W. S.

PROCEEDINGS BOSTON SOC. NAT. HIST., 1860.—p. 226, On two Birds from Bogota —*Turdus minimus* (Lafresnaye) and *Vireo Bogotensis* (Bryant); *Dr. Henry Bryant.* —p. 227, Fossiliferous slate and sandstone (Devonian) from the Dennis river, Maine; *Prof. W. B. Rogers.*—p. 228, Spines of Siluroid fishes from a whale's blubber; *Capt. Atwood.*—p. 229, "Cocoa-nut pearl;" *Dr. C. F. Winslow.*—p. 229, Discussion on the subject of Vision; *Dr. J. B. Jeffries, Prof. W. B. Rogers,* and *Dr. Gould*—p. 231, Discussion on the theory of Darwin; *Agassiz, Emerson,* and *Prof. W. B. Rogers.*— p. 235, Description of Hoplocampa rubi, by the late *Dr. T. W. Harris;* with remarks on its history by *Noyes Darling, Esq.; Mr. Scudder.*—p. 236, Geological map of Vermont; *Mr. C. H. Hitchcock,* with remarks by *Prof. W. B. Rogers.*—p. 239, Section of an elephant's tooth as an example of *Osteo-dentine; Dr. White.*—p. 240, Laws of fracture of a thick glass tube; *Prof. W. B. Rogers.*

THE

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[SECOND SERIES.]

ART. XV.—*On the Nebular Hypothesis;* by Professor DANIEL KIRKWOOD, Bloomington, Indiana.

THE records of our planet's physical history, from the dawn of organized existence down to the epoch of man's creation, have, for the most part, been brought to light since the commencement of the nineteenth century. Within this brief period the immense antiquity of our globe, the former high temperature of its surface, and, in short, all the main facts and doctrines of geology, have been discovered or confirmed by a laborious examination of the planetary crust. *But the earth has also a pre-geologic history*—a history as yet undeveloped; the grand outlines of which must be derived chiefly from *celestial* phenomena. "The testimony of the rocks" in regard to the one, may be more explicit than that of the *stars* in regard to the other. It must be borne in mind, however, that this department of research—the tracing of astronomical facts not accounted for by gravitation, to their source in the origin of the system—has hitherto received but little attention from men of science. Cogent arguments, it is true, were adduced by Laplace and Pontécoulant in favor of the nebular hypothesis, but very little has since been accomplished, tending either to invalidate or confirm it.

The present article is designed as a popular rather than a scientific discussion of this interesting subject, and we trust its interest will not be abated by the fact that a portion of the matter has been before presented by us anonymously in a Quarterly Review. We shall in the first place present a brief account of the origin and nature of Laplace's theory; secondly, a connected

view of the principal phenomena by which it is sustained; and thirdly, consider the most prominent of the objections which have been urged against it.

As a group, our solar system is comparatively isolated in space; the distance of the nearest fixed star being at least seven thousand times that of Neptune, the most remote known planet. Besides the central or controlling orb, it contains, so far as known at present, sixty-seven primary planets, twenty-one satellites, three planetary rings, and nearly eight hundred comets. In taking the most cursory view of this system of bodies, we cannot fail to notice the following interesting facts in regard to the motions of its various members:—

1. The sun rotates on his axis from west to east.

2. The primary planets all move nearly in the plane of the sun's equator.

3. The orbital motions of all the planets, primary and secondary, except the satellites of Uranus and perhaps those of Neptune, are in the same *direction* with the sun's rotation.

4. The direction of the rotary motions of all the planets, primary and secondary, in so far as has been observed, is identical with that of their orbital revolutions; viz., from west to east.

5. The rings of Saturn revolve about the planet in the same direction.

6. The planetary orbits are all nearly circular.

7. The *cometary*, is distinguished from the *planetary* portion of the system by several striking characteristics: the orbits of comets are very eccentric and inclined to each other, and to the ecliptic at all possible angles. The motions of a large proportion of comets are *from east to west*. The physical constitution of the latter class of bodies is also very different from that of the former; the matter of which comets are composed being so exceedingly attenuated, at least in many instances, that fixed stars have been distinctly visible through what appeared to be the densest portion of their substance.

None of these facts are accounted for by the law of gravitation. The sun's attraction can have no influence whatever in determining either the *direction* of a planet's motion, or the eccentricity of its orbit. Iu other words, this power would sustain a planetary body moving from east to west, as well as from west to east; in an orbit having any possible degree of inclination to the plane of the sun's equator, no less than in one coincident with it; or, in a very eccentric ellipse, as well as in one differing but little from a circle. The consideration of the coincidences which we have enumerated led LaPlace to conclude that their explanation must be referred to the *mode* of our system's formation—a conclusion which he regarded as strongly confirmed by the contemporary researches of Sir William Herschel. Of the numer-

ous nebulæ discovered and described by that eminent observer, a large proportion could not, even by his powerful telescope, be resolved into stars. In regard to many of these, it was not doubted that glasses of superior power would show them to be extremely remote sidereal clusters. On the other hand, a considerable number were examined which gave no indications of resolvability. These were supposed to be a species of self-luminous, nebulous matter—the chaotic elements of future stars. The great number of these irresolvable nebulæ, scattered over the heavens and apparently indicating the various stages of central condensation, very naturally suggested the idea that the solar system, and perhaps every other system in the universe, originally existed in a similar state. The sun was supposed by Laplace to have been an exceedingly diffused, rotating nebula, of spherical or spheroidal form, extending beyond the orbit of the most distant planet; the planets as yet having no separate existence. This immense sphere of vapor, in consequence of the radiation of heat and the continued action of gravity, became gradually more dense, which condensation was necessarily attended by an increased angular velocity of rotation. At length a point was thus reached where the centrifugal force of the equatorial parts was equal to the central attraction. The condensation of the interior meanwhile continuing, this zone was detached, but necessarily continued to revolve around the central mass with the same velocity that it had at the epoch of its separation. If perfectly uniform throughout its entire circumference, which would be highly improbable, it would continue its motion in an unbroken ring, like that of Saturn; if not, it would probably collect into several masses, having orbits nearly identical. "These masses should assume a spheroidal form, with a rotary motion in the direction of that of their revolution, because their inferior particles have a less real velocity than the superior; they have therefore constituted so many planets in a state of vapor. But if one of them was sufficiently powerful, to unite successively by its attraction, all the others about its centre, the ring of vapors would be changed into one spheroidal mass, circulating about the sun, with a motion of rotation in the same direction with that of revolution."* Such, according to the theory of Laplace, is the history of the formation of the most remote planet of our system. That of every other, both primary and secondary, would be precisely similar.

If it be said that the small eccentricities of the planetary orbits, the approximate coincidence of their planes with that of the solar equator, and the uniformity of direction in which the planets move, are *ultimate facts* that the *final cause* of these arrange-

* Harte's Translation of Laplace's System of the World, vol. ii, note vii.

ments was the stability of the system; and that we transgress the legitimate domain of scientific research in *attempting* their explanation;—we ask by what rule in philosophy the arrangements in question are determined to be ultimate facts. Even granting their final cause to have been the stability of the system, we are by no means to conclude that they are necessarily unsusceptible of explanation. "Final causes," says Whewell,* "are to be excluded *from physical inquiry*; that is, we are not to assume that we know the object of the Creator's design, and put this assumed purpose in the place of a physical cause. We are not to think it a sufficient account of the clouds that they are for watering the earth—to take Bacon's examples—or 'that the solidness of the earth is for the station and mansion of living creatures.' The physical philosopher has it for his business to trace clouds to the laws of evaporation and condensation; to determine the magnitude and mode of action of the forces of cohesion and crystallization by which the materials of the earth are made solid and firm. This he does, making no use of the notion of final causes; and it is precisely because he has thus established his theories independently of any assumption of an end, that the end, when, after all, it returns upon him, and cannot be evaded, becomes an irresistible evidence of an Intelligent Legislator. He finds that the effects, of which the use is obvious, are produced by most simple and comprehensive laws; and when he has obtained this view, he is struck by the beauty of the means, by the refined and skillful manner in which the useful effects are brought about;—points different from those to which his researches were directed."

As the question, then, to which the cosmogony of Laplace proposes a solution, is a legitimate one, we shall proceed to consider some of the evidences by which the theory is supported.

1. *The nebular hypothesis furnishes a very simple explanation of the motions and arrangements of the planetary system.* In the first place, it is evident that the separation of a ring would take place at the equator of the revolving mass, where of course the centrifugal force would be greatest. These concentric rings—and consequently the resulting planets—would all revolve *in nearly the same plane.* It is evident also that the central orb must have a revolution on its axis *in the same direction with the progressive motions of the planets.* Again: at the breaking up of a ring, the particles of nebulous matter more distant from the sun would have a greater absolute velocity than those nearer to it, which would produce the observed *unity of direction in the rotary and orbital revolutions.* The motions of the satellites are explained in like manner. The hypothesis, moreover, accounts satisfactorily for the fact that the orbits of the planets are all nearly circular.

* Bridgewater Treatise, vol. ii, p. 180.

And finally, it presents an obvious explanation of the rings of Saturn. It would almost seem, indeed, as if these wonderful annuli had been left by the Architect of Nature, as an index to the creative process.

The argument derived from the motions of the various members of the solar system is not new, having been forcibly stated by Laplace, Pontécoulant, Nichol, and other astronomers. Its full weight and importance, however, have not, we think, been duly appreciated. That a common physical cause has determined these motions, must be admitted by every philosophic mind. But apart from the nebular hypothesis, no such cause, adequate both in mode and measure, has ever been suggested;—indeed none, it seems to us, is conceivable. The phenomena which we have enumerated *demand* an explanation, and this demand is met by the nebular hypothesis. It will be found, therefore, when closely examined, that the evidence afforded by the celestial motions is sufficient to give the theory of Laplace a very high degree of probability.

2. The fact that this theory of the genesis of the solar system may be extended to the binary and multiple systems among the so-called fixed stars, may be urged as no inconsiderable evidence in its favor. Unity, no less than variety, is characteristic of nature's works. All the diversified and apparently disconnected phenomena of the universe have their roots in a few general laws. Whatever, therefore, leads us higher in the process of generalization may be presumed to have some foundation in truth.

3. Numerous geological facts appear to harmonize with the hypothesis under consideration. Fossil organic remains, and their absence in the earlier rocks, both indicate that the temperature of the earth's surface was formerly much higher than at present, and that the decrease was slow and gradual. In places, for instance, where ferns do not now exceed three feet in height, those of former periods are found to have been from forty to fifty, or even seventy feet high. Now in regard to existing plants, it is well-known that their *number*, as well as their *size and luxuriance of growth*, gradually increase as we advance from high latitudes towards the equator. In both these respects a similar increase is observable in descending through the successive strata of the ancient world, until we reach the oldest Secondary rocks. These facts are doubtless to be referred to the same cause, namely, a gradual change of temperature. A comparison of fossil animals with those now existing, leads to a similar result. This high temperature of the earth's surface during the earlier stages of its physical history, is attributed by most geologists to a central heat,* which diffused itself through-

* A different theory in regard to the ancient high temperature of the earth has been developed by the celebrated Poisson. Starting with the fact—established be-

out the entire mass. That the interior of our planet is in a state of igneous fluidity, or has, at least, an extremely high temperature, is now very generally admitted. As we descend from the surface we find a regular increase of heat, varying indeed for different localities, partly no doubt, on account of the different conducting powers of the rocks which constitute the crust, but averaging about one degree for every fifty feet.* This has been ascertained from a great variety of experiments upon rocks and springs in mines, and upon the water issuing from Artesian wells. It is true, the depth to which man has been able to penetrate, is comparatively small; but assuming the same rate of increase to continue, it has been calculated that the point at which all known rocks would be in a state of fusion must be considerably less than one hundred miles from the surface, and possibly less than fifty. Reasons are not wanting for regarding this molten mass as the seat of present volcanic activity,† as well as the source of that great upheaving power by which not only mountains, but islands and continents have been elevated.

Geologists are generally agreed that there is conclusive evidence of the primitive igneous fluidity of the unstratified rocks of the earth's crust. Indeed it can scarcely be doubted that the solidification of the outer portions of our globe was a mere cooling by radiation from the surface. "There is no small reason," says Professor Hitchcock, "to suppose that the globe underwent numerous changes previous to the time when animals were placed upon it; that, in fact, the time was when the whole matter of

yond doubt by modern observations—that the solar system has a progressive motion in space, he supposes that in the sweep of its mighty orbit it has passed through regions of very different temperatures, and that the heat of former periods—the residuum of which is still found at great depths beneath the surface—was received *ab extra* while in a portion of space much richer in stars, and having therefore a higher temperature, than that through which it is at present passing. "The physical doubts which have reasonably been entertained against this extraordinary cosmical view, (which attributes to the regions of space that which probably is more dependent on the first transition of matter condensing from the gaseo-fluid into the solid state,) will be found collected in Poggendorff's *Annalen*, bd. xxxix, S. 93—100." Humboldt's Cosmos, vol. i. p. 165.

* See Proceedings of the American Association for the Advancement of Science, Tenth Meeting, Part II. p. 102.

† "The similarity of lava, wherever found, and the close agreement as to composition and physical characters of the basalt of ancient epochs and of that still bursting through and intersecting the walls of modern volcanoes, are proofs that all such eruptions have a common origin, and are due, as well as the accompanying physical phenomena of earthquakes, to forces acting on the still liquid portion of the earth." *Portlock's Rudimentary Geology*, p. 70.

The researches of Hopkins and Hennessy have led to different results in regard to the thickness of the earth's crust. See Trans. of the Roy. Soc. for 1839, p. 381; for 1840, p. 193; for 1342, p. 43; for 1851, Part II, p. 495. Also, this Journal for March, 1852, p. 271, and for May, 1853, p. 126. For "some suggestions in Explanation of the primitive Incandescent condition of the Earth and the other Planets," see Monthly Notices of the Roy. Astro. Soc. for January 13th, 1854.

the earth was in a melted and not improbably also in a gaseous state."* All this, it will be perceived, is in striking accordance with the Nebular Hypothesis.

4. Whatever may be the nature of the elevating force, it is not confined to our globe. The lunar and planetary mountains afford evidence of its action in the other members of our system—a fact which seems indicative of their common origin.

5. The spheroidal figure of the planets points to a great and significant fact in regard to their primitive constitution—the fact that they have *all*, at former epochs in their history, been in a liquid, if not in a gaseous state. That the polar flattening of the earth is not conclusive evidence of its primitive fluidity, has been affirmed, we are aware, by more than one writer of scientific eminence. If we suppose our planet to have been originally solid and perfectly spherical—its surface being entirely covered with water—the effect of its axial revolution would be the accumulation of water in the equatorial region, and a consequent recession from the poles. Sir John Herschel remarks,† that the gradual abrasion of these polar continents and their slow deposition in the deep equatorial ocean would eventually reduce the solid earth itself to the form of an oblate spheroid. Recently, however, H. Hennessy, an eminent mathematician, has subjected this hypothesis to the best of scientific scrutiny. The learned and original researches of this gentleman have shown that the ultimate ellipticity, in case the earth were at first a *solid* sphere, as supposed by Herschel, would be $\frac{1}{464}$, while that found by actual measurement is $\frac{1}{292}$ to $\frac{1}{302 \cdot 064}$.‡ The theory of primitive fluidity may therefore be regarded as fully established. It is worthy of remark that the oblateness of Mars is much greater than would have resulted from its observed velocity of rotation, supposing the planet to have been originally fluid, homogeneous, and of spherical form. How is this anomaly to be accounted for? On the nebular hypothesis we have only to suppose that in the process of transition from the gaseous to the liquid and solid form, "the liquid surface of some planets was solidified before they could assume the figure appertaining to their velocity of rotation."§

6. The Nebular Hypothesis affords the most probable explanation of the phenomena of comets. Laplace supposed these bodies to have had their origin in portions of nebulous matter which had been left about the points of equal attraction between the sun and neighboring stars—the occasional preponderance of the solar influence causing portions of these outstanding nebulosities to enter our system from different quarters of the heavens. The orbits of such bodies would be very eccentric, and might have

* Religion of Geology, p. 22. † Outlines, Arts 226, 227. ‡ Mädler, p. 50, this vol.
§ Humboldt's Cosmos, vol. iv, (Bohn's Edition,) p. 427.

any degree of inclination to the plane of the ecliptic. Their motion, moreover, might be either direct or retrograde. Thus the great characteristics which distinguish the members of the cometary from those of the planetary system are necessary consequences of this theory of their origin.

"The attraction of the planets," says Laplace, "and perhaps also the resistance of the ethereal medium, ought to change several cometary orbits into ellipses, of which the greater axes are much less than the radius of the solar activity. It is probable that such a change was produced on the orbit of the comet of 1759, the greater axis of which was not more than thirty-five times the distance of the sun from the earth. A still greater change was produced in the orbits of the comets of 1770, and of 1805."*

7. The evidence afforded by the telescope in regard to the present physical constitution of the different members of the solar system appears to be confirmatory of the nebular hypothesis. If the sun and planets have condensed from a gaseous state, the former, not only on account of the comparative recentness of its formation, but especially because of its great relative magnitude would retain an intensely high temperature for an indefinite period after the planets had cooled down. *The present heat, therefore, of the central orb, and the existence of a very extensive gaseous envelope, together with the various phenomena of the solar spots, would result as consequences from the theory of Laplace.*

The Moon.—Although the diameter of the earth is nearly four times that of her satellite, the mountain elevations of the latter are nearly equal to those of the former. The cup-shaped cavities, which cover a great part of the lunar surface, are very different in appearance from volcanoes on the earth. The largest are from fifty to one hundred and fifty miles in diameter, and some are from three to four miles deep. In no case, however, is there any decisive evidence of *present* volcanic activity, though doubtless the phenomena justify the inference that at some remote epoch in the past history of our satellite, its crust has been agitated and shattered by up-heaving eruptive forces, compared with which the similar agencies in our own planet, at least those in operation during the historical period, sink into insignificance.

In contemplating these striking characteristics of the moon's surface—the traces of ancient igneous activity over the whole visible hemisphere, and the apparent absence of organization—it is natural to inquire—what is their physical import? If our satellite, in accordance with the nebular hypothesis, has solidified from a gaseous state, the earth having also undergone the same process, the latter, on account of its superior magnitude, would require a much longer period than the former to cool down from

* System of the World, Harte's Translation, vol. ii, p. 364.

a condition of igneous fluidity. The moon may therefore be farther advanced in its physical history than the earth. On this subject Mr. Nasmyth, of Manchester, England, who has devoted much time and attention to observations on the lunar surface, remarks as follows:—"Having in my travels, seen the actual results of volcanic action, extinct and active, I think I can comprehend what I observe on the moon, and trace the analogy where it is applicable. * * * I may express one of my most definite and strong convictions in a few words, namely, *that I do not believe there is one of the countless thousands of volcanoes, whose craters bespatter the lunar surface, in action, or which has been in action, for thousands of ages past.* I am vain enough to think I have got the right view of the true nature of volcanic action; a view which close observation of the phenomenon, in all its phases, has impressed on me; namely, *that volcanic action is an expiring phenomenon*, having for its cause and source great cosmical principles, quite independent from any mere chemical action; and, in that view, I consider molten lava, and the heat of volcanic action, to be nothing less than the residue of that *igneous* state through which *all* the planets have passed, in their cosmical history, from the earliest moment of their creation to the present time. And, in this view, if our globe be permitted to exist, in its present condition, for ages to come, volcanic action, as an active phenomenon will dwindle away, and finally cease to exist,—the solid crust of the earth so increasing in thickness as to prevent the issue of any of the yet remaining molten matter from its interior.

"The moon, from its small mass, and proportionally great surface, must have cooled down much more rapidly than the earth; and all have been dead, tranquil and silent, for countless ages, ere we had passed over our rampant volcanic era, of which our most tremendous modern volcanoes are but mole-hills in comparison."

Jupiter and Saturn.—What indications do the belts of Jupiter and Saturn afford in regard to the present physical condition of these planets? When our own planet was in a molten state, where was the water which now constitutes our oceans? It must necessarily have existed in the form of vapor, entirely surrounding the earth, and at a considerable distance from it. Nor could this water descend to, and remain permanently upon the surface, until long after the solidification of the crust by cooling. During this "pre-oceanic" period, our globe, it is likely, would have appeared to a distant spectator, very much as Jupiter and Saturn with their cloudy belts now appear to us. Now it has been justly remarked, that "the length of time which would be required for such enormous planets as Jupiter and Saturn to cool down from their original molten and incandescent condition to such a

temperature as would be fitted to permit their oceanic matter permanently to descend and remain upon the surface, *would be vastly longer than in the case of such a comparatively small planet as the earth.*"

Thus, in accordance with the nebular hypothesis, it would seem that Jupiter and Saturn, on account of their vast dimensions, still retain so much of their primitive heat as to produce the vaporous envelopes by which they are surrounded.

Variable Stars.—The well-known phenomena of variable stars, the explanation of which has greatly perplexed astronomers, is ingeniously accounted for, according to the nebular hypothesis, by Mr. A. D. Wackerbarth, as follows: "Suppose a nebula such as that from which the earth, sun, and planets, are supposed to have arisen, existing in space, such a nebula would probably be composed of elements more or less the same as those whereof our own planet is formed.* Some, indeed, of the latter we might suppose wanting, and others present which we possess not here; but on the whole, let us suppose that the chief elements of the earth are found in our nebula, which would thus form an immense spheroid of nebulous matter revolving round its own minor axis; or rather, if that matter were not quite homogeneously distributed, on an axis passing through its centre of gravity. We may suppose or not as we please, that this nebula has a nucleus, as many nebula appear to have, and many not to have, any such portion; but in the former case we must suppose some little difference in the constitution of the particles towards the centre, or position of the said nucleus. Our nebula, thus composed, may wander a longer or shorter period in space peacefully; but now let us suppose a disturbance such as that which broke up the nebulosity of the mass which forms the planetary system, and condensed it into separate globes. Such disturbance might come from without or from within; there are forces in nature to account for either. We have supposed all or many of the elements present; but in a nebulous form they would be in a finely divided state, and many of them, perhaps all the baser metals, have such affinity to oxygen, as when in that state, to take fire on coming in contact therewith; so that any cause, which would bring them and the oxygen into contact, would cause fearful explosions, and set the whole nebula in

* It is a remarkable fact that no *new* elements have been found in meteoric stones, which are now regarded as planetary bodies. "I would ask," says Humboldt, "why the elementary substances which compose one group of cosmical bodies, or one planetary system, may not in a great measure be identical? Why should we not adopt this view, since we may conjecture that these planetary bodies, like all the larger or smaller agglomerated masses revolving round the sun, have been thrown off from the once far more expanded solar atmosphere, and been formed from vaporous rings describing their orbits round the central body?" (Cosmos, vol. i, p. 120.) Mr. Wackerbarth extends this hypothesis in regard to the identity of the elementary substances to *stellar systems.*

ignition, condensing it into burning masses, each by the violent explosions casting out smaller fused and burning masses into space, to revolve as burning globes around itself, or the centre of gravity of the whole. But what would be the condition of one of these fused and blazing masses? Hydrogen is present as well as oxygen; and ignition must therefore immediately cause the production of water. An ocean is thus poured down on the incandescent globe, to be cast up again as steam into a damp atmosphere formed by the nitrogen and the watery vapor. Here it cools, and is again poured down in torrents on the glowing mass; and this process must continue until the globe has sufficiently cooled for the water to be able to rest upon its surface. Now, I imagine that the appearance of a globe in such a state viewed at a stellar distance, must be variable; that when the water is, in the form of steam, driven up into the atmosphere, the burning mass must glow with greater brilliancy; but when, the steam condensing, a boiling ocean is poured upon it, the violence of the conflagration must for a time abate, and thus the object assume a less brilliant appearance, until the fire has succeeded in reconverting the water into steam, and driving it up again into the atmosphere.

"Another circumstance may be mentioned as possible, namely, some bodies may be at present in some parts extinct; while other parts of them are yet fused and burning. Would not the revolution of such a body present the phenomena of a variable star?"*

8. *The Satellites.*—It is a remarkable coincidence in regard to the motions of the moon that her rotation is completed in a period precisely equal to that of her orbital revolution. The same is true of Jupiter's satellites, at least *some* of Saturn's, and probably, indeed, of all secondaries. Such coincidences are not to be ascribed to chance. Either, therefore, we must regard them as *ultimate facts*, or refer them to the operation of those primitive natural forces by which other phenomena of the heavenly bodies were produced. Few correct thinkers, we presume, would be inclined to adopt the former alternative. "The craving of the philosophic mind is for *explanation*, *i. e.*, for the breaking up of complex phenomena into familiar sequences, or equally familiar transitional changes, or contemporary manifestations."† There can be no doubt that the equality in question is due to the operation of known physical forces. Sir Isaac Newton's explanation,‡ it is well-known, assumes the original fluidity of the satellites. The attraction of the earth on the primitive fluid mass of the moon would produce an elongation of the hemisphere in the

* Monthly Notices of the Roy. Astr. Soc. for May 9th, 1856.

† Edinburgh Review, No. 175. ‡ Principia, B. iii, Prop. xxxvii, Cor.

direction of the primary. The force of gravity of this tidal elevation would maintain the greatest axis in the direction of the attracting body, thus producing the observed coincidence. The same applies to the other secondary planets.

This equality, we believe, is best explained by the nebular hypothesis. Newton's explanation pre-supposes the two motions to have been originally so adjusted as to differ *very little* from exact coincidence. But if the moon once existed in a state of vapor, its volume was much greater than at present, and the gravity of particles at its surface proportionally less; while from the fact of their gaseous condition they would yield with the greatest facility to any force impressed upon them. The equality between the two motions might thus become established long before our satellite had contracted to its present dimensions.

But why, it may be asked, should this isochronism obtain universally in the subordinate systems, while there is not the least approximation to it in the case of any primary planet? It has been suggested that "the integrity of the motions of the ring, when it resulted in satellites, may have arisen from the comparative maturity of the system—then approaching the close of the first epoch in its history."* Without entering here into any discussion of this question, we may remark that in the case of the secondary planets, *the epoch of solidification would evidently be reached, and consequently the acceleration of the rotary velocity arrested, at a comparatively early period of their existence.* "The motions of the three first satellites of Jupiter present a phenomenon still more extraordinary than the preceding; which consists in this, that the mean longitude of the first, minus three times that of the second, plus twice that of the third, is constantly equal to two right angles. There is the ratio of infinity to one, that this equality is not the effect of chance. But in order to produce it, it is sufficient, if at the commencement, the mean motions of these three bodies approached very near to the relation which renders the mean motion of the first, minus three times that of the second, plus twice that of the third, equal to nothing. Then their mutual attraction, rendered this ratio rigorously exact, and it has moreover made the mean longitude of the first, minus three times that of the second, plus twice that of the third, equal to a semi-circumference. At the same time, it gave rise to a periodic inequality, which depends on the small quantity, by which the mean motions originally deviated from the relation which we have just announced. Notwithstanding all the care Delambre took in his observations, he could not recognize this inequality, which, while it evinces its extreme smallness, also indicates, with a high degree of probability, the existence of a cause which makes it disappear. In our hypothesis,

* Dr. Nichol.

the satellites of Jupiter, immediately after their formation, did not move in a perfect vacuum; the less condensible molecules of the primitive atmospheres of the sun and planet would then constitute a rare medium, the resistance of which being different for each of the stars, might make the mean motions to approach by degrees to the ratio in question; and when these movements had thus attained the conditions requisite, in order that the mutual attraction of the three satellites might render the relation accurately true, it perpetually diminished the inequality which this relation originated, and eventually rendered it insensible. We cannot better illustrate these effects than by comparing them to the motion of a pendulum, which, actuated by a great velocity, moves in a medium, the resistance of which is inconsiderable. It will first describe a great number of circumferences; but at length, its motion of circulation perpetually decreasing, it will be converted into an oscillatory motion, which itself diminishing more and more, by the resistance of the medium, will eventually be totally destroyed, and then the pendulum, having attained a state of repose, will remain at rest forever."*

9. *The Asteroids.*—Our data perhaps may not yet be sufficient to afford a satisfactory explanation of the phenomena of the asteroids. The question, however, in regard to their origin, and the bearing of this inquiry on the cosmogony of our system, are subjects of more than ordinary interest. The following facts appear to indicate the most plausible theory of their origin:—

(1.) These bodies occupy a chasm—observed before their discovery—in the order of the planetary distances; an order which indicated the existence of a *single planet* where *fifty-eight* have already been detected.

(2.) The members of the group are characterized by certain peculiarities which are doubtless indicative of an intimate mutual relationship. They are extremely diminutive in size; the volume of the largest probably not exceeding the ten-thousandth part of that of the earth. The eccentricities and inclinations of their orbits are generally much greater than those of the other planets. Their orbits interlace. "The strongest evidence," says D'Arrest, "of the intimate connection of the whole group of small planets appears to be, that if the orbits are supposed to be represented materially as hoops, they all hang together in such a manner that the entire group may be suspended by any given one." The occasional rapid variation in the apparent magnitude of several members of the group—changes not attributable to variations of distance—would seem to indicate some peculiarity in their physical constitution.

(3.) The asteroids already discovered amount to one for about every six degrees of longitude. Their number appears to *in-*

* Harte's Translation of Laplace's System of the World, vol. ii, p. 367.

crease with the *decrease* of their apparent magnitude. In all probability hundreds of these bodies will have been discovered by the close of the present century; and the existence of immense numbers too small to be detected, cannot reasonably be doubted. In fact, the zone is even yet almost entitled to the appellation of a *primary planetary ring.*

(4.) Should the rings of Saturn, (now regarded by some astronomers, as fluid,) from any cause whatever become separated into parts, and collect about distinct nuclei, a ring of *secondary asteroids* would be formed, analogous to the zone between Mars and Jupiter.

(5.) It has been affirmed by an eminent astronomer that the rings of Saturn are sustained by the direct action of the satellites; that no other planet of the system has such an arrangement of secondaries as to secure the stability of a ring; and that the only place in the solar system where a primary ring would be long sustained, is the region occupied by the small planets.

(6.) The amount of perturbation in this point of the system is extraordinary in consequence of its proximity to the enormous mass of Jupiter.

Now according to the nebular hypothesis each of the principal planets originally existed as a gaseous ring. The observed order would require such an annulus between Mars and Jupiter, and this is the precise situation in which a ring would be the longest sustained by the exterior planets. Upon the breaking up of the ring, however, *a zone of small planets would naturally be formed* unless some one portion of the vaporous mass should have a preponderating influence so as ultimately to absorb all the rest. In short, it is believed that the nebular hypothesis will explain the various phenomena of these bodies, which without it seem inexplicable.

10. *The Zodiacal Light.*—Cassini regarded the zodiacal light as produced by an innumerable multitude of small planetary bodies revolving in a ring about the sun. This hypothesis was generally accepted until 1855, when another was proposed by the Rev. George Jones, of the United States Navy. Mr. Jones thinks his own observations of the phenomena wholly incompatible with the hypothesis of a nebulous ring *with the sun for its centre*, and also with that of a nebulous planetary body revolving round the sun. He is led, therefore, to the alternative of a *nebulous ring round the earth, interior to the moon's orbit.* This theory has been favorably received by the astronomers of our country. Objections of some weight, however, have been urged against it by Pres. F. A. P. Barnard,* of the University of Mississippi, and it will not perhaps be generally adopted without further confirmation. But it is generally admitted by the advocates

* This Journal for March, 1856.

of each hypothesis that the appearance is produced by a *continuous zone of infinitesimal asteroids.* Now if the abandoned rings of either the *solar* or the primitive *terrestrial* atmosphere contained any matter of too volatile a nature to coalesce in the formation of solid planetary bodies, such molecules ought to exhibit the appearance of a flattened ring, of a vapory or nebulous form, like that actually presented by the zodiacal light. Whether we regard it, therefore, as a primary or secondary ring, the theory of Laplace affords the most plausible explanation of its origin.

11. *The Rotations of the Planets.*—Is there or is there not any bond of union between the periods of rotation of the different members of our system? That these rotations should be mere isolated facts, independent of all order and relationship, seems, to say the least, extremely improbable. Even in the process of crystallization the atoms of matter arrange themselves in regular mathematical forms; the chemical combination of elements occurs only in definite proportions; the *orbital* motions of the planets are governed by invariable laws; in short, order and harmony appear to characterize all the operations of nature around us, or in the language of an ancient philosopher, "God geometrizes." Is there any probability that we have an exception to this general prevalence of law, in the *rotations* of the heavenly bodies?

The first attempt, it is believed, to develop a connection between these apparently independent elements, was undertaken by the writer in 1839. The result of these researches was the discovery of an interesting planetary harmony, which, in 1849, was communicated to the American Association for the Advancement of Science.* It is not claimed for this Analogy that it has been fully established as a physical law. It appears, however, to harmonize too closely with the known elements of the solar system, to be regarded as merely an accidental coincidence. Now the immediate dependence of one of the elements of this analogy—viz., the diameter of the sphere of attraction—on the nebular hypothesis, is obvious. It is evident also that coincidence between the periods of rotation and revolution is a necessary characteristic of planetary rings revolving around a central mass, and that on the transformation of a ring into a planet the same equality would obtain when the *diameter* of the latter was equal to the *breadth* of the former. Any further condensation, however, would *increase* the rotary velocity, and thus shorten the period of axial revolution. Hence *the ratio of the angular velocity of rotation to that of orbital revolution, or what is the same thing, the number of a planet's days in its year, would be a function of the diameter of the sphere of attraction.*

12. *The distribution of the satellites.*—Another interesting con-

* See Proceedings of the Am. Assoc., for 1849, p, 207.

sideration may here be suggested. Planets of greater magnitude and having greater spheres of attraction would probably throw off the greater number of rings. An explanation may thus be given of the striking fact in the constitution of the solar system, that the exterior planets have numerous satellites. Saturn, which has the greatest sphere of attraction, has the greatest number of satellites; Uranus, the next in order in this respect, has the next greatest number; while Jupiter, which is the *third* in regard to the former, is *third* also in the latter. Mars, it is true, has a greater sphere of attraction than the earth; it may be remarked, however, that owing to the small size of the planet, and its proximity to Jupiter, a satellite would not be *stable* unless very near the primary.

13. *Recapitulation—Simplicity of the Nebular Hypothesis.*— When the number and variety of the phenomena explained by the nebular hypothesis are duly considered, the weight of evidence in its favor must be admitted, we think, by every unbiassed mind. The rotation of the sun; the unity of direction of the planetary motions; the approximate coincidence of the planes in which the planets move, with the plane of the solar equator; the general agreement between the direction of the axial and orbital revolutions; the small eccentricities of the planetary orbits; the rings of Saturn; the central heat of the earth; the oblate form and primitive fluidity of the planets; the origin of comets; telescopic revelations in regard to the physical constitution of the sun, moon, and some of the planets; the phenomena of variable stars; the equality between the periods of rotation and orbital revolution of the satellites; the extraordinary phenomena presented by the first three satellites of Jupiter; the zone of asteroids between Mars and Jupiter; the zodiacal light; the analogy between the periods of rotation of the primary planets; the distribution of the satellites;—all, as we have seen, are accounted for by the nebular cosmogony, while for many of them at least, no other explanation has ever been offered. Adopting this hypothesis, all the motions of the solar system are derived from a *single impulse* communicated by the Creator to the primitive nebula: rejecting it, each motion of every member demands the *separate* operation of his power. Now, "if there be two modes of explaining any phenomenon of nature, then, *cæteris paribus*, that is the most probable which is the most simple. For by what we observe in the creation around, we are forced upon the conviction that the Almighty acts in this respect with that *economy* of creative energy, which, although infinitely more perfect in its degree, has nevertheless its visible type in that husbandry of *our* resources, that disposition to economy in *our* efforts, which impels us always to avail ourselves of the simplest possible means of effecting all that we wish to do.

"Thus, when in reasoning upon any hypothesis, we are forced back upon secondary causes, it is sound philosophy to judge of the probability of that hypothesis, according to the simplicity or complication of the causes to which we are thus compelled ultimately to refer it. If, for instance, there be two hypotheses, by one of which we shall be compelled to fall back upon a double operation of the hand of the Almighty, whereas the other resolves it into a single effort of his will, then is the latter hypothesis, according to the analogy of nature, more probable than the former, and that infinitely."

Objections to the Nebular Hypothesis.

1. *The Satellites of Uranus.*—It has been objected to the nebular hypothesis that it cannot be reconciled with the retrograde motion of the satellites of Uranus. We reply that in every instance, so far as we know, the motions of secondary planets are performed in planes nearly perpendicular to the axis of the primary, but that in no case, with the single exception of Jupiter, is the axis of a primary planet nearly perpendicular to the plane of its own orbit. The earth's axis is inclined more than twenty-three degrees; that of Mars, as well as that of Saturn, nearly thirty; while that of Venus, if we may rely on the observations of Schroeter, is inclined more than seventy degrees. Now if this amount of inclination does not invalidate the hypothesis in question, ought the greater one of one hundred degrees—which gives the satellites of Uranus their backward motion—to be regarded as wholly inconsistent with it? These inclinations, as well as the slight eccentricities of the orbits, and their deviations from the plane of the sun's equator, may be ascribed partly to irregularities in the density and temperature of the planets in a state of vapor, and to the operation of molecular forces during the process of condensation. The following suggestion of Laplace is also worthy of note:—"If any comets have penetrated the atmospheres of the sun and planets at their formation, they must have described spirals, and consequently fallen on the bodies, and in consequence of their fall, caused the planes of the orbits and of the equators of the planets to deviate from the plane of the solar equator."* "The probabilities of the case," says Professor Nichol, "certainly favor the idea, that it [the anomalous motion of the Uranian satellites] is a *disturbance*, an *interference* with the *order* of the system, by some *foreign* law or occurrence."†

But while the hypothesis requires *in general* that the direction of the planetary rotations, as well as that of the orbital motions of their satellites, should be the same as that of their revolution

* System of the World, Harte's Translation, vol. ii, p. 364.

† Thoughts on some Important Points Relating to the System of the World. First American Edition, p. 109.

round the sun, an *opposite* direction is not, as some have supposed, incompatible with it. Perhaps, indeed, it is not more improbable than a permanent ring, like that of Saturn, or a zone of small planets like the asteroids. We have supposed that on the breaking up of a nebulous ring its matter collected into several masses, the orbits of which were not greatly different, and that they revolved separately during an indefinite period, but ultimately united, except in the case of the asteroids, in *one* planetary body. Those primitive masses, owing to perturbations, &c., would meet in different directions, and it is by no means impossible that the circumstances of the collision and coalescence of the nebulous fragments may have been such as to overcome the natural tendency to rotate in the direction of the orbital motion.

Again: the form of the primary ring may have influenced the original *direction* of the planet's rotation, and hence that of its *secondary* rings and the resulting satellites. If the thickness of a ring was greatest in the direction at right angles to the plane of its motion, or if the diameter of its mass when it first assumed the form of a gaseous planet, was greatest in that direction, the plane of rotation would probably, for obvious reasons, be inclined to that of the orbit; the amount of inclination depending upon the ratio of the diameters.*

2. *The Revelations of Lord Rosse's Telescope.*—The opponents of the nebular hypothesis affirm that it derived its chief support from the supposed existence of irresolvable nebulæ; that is, of widely diffused vaporiform matter not yet aggregated into stars, but slowly undergoing the process of condensation. Such, until 1846, was believed to be the constitution of a large proportion of those mysterious objects. The great nebula in Orion, for instance, was thought to present decided indications of irresolvability. Lord Rosse's telescope, however, has shown it to consist at least in some portions, of minute stellar points, in extremely close proximity; in the language of Dr. Nichol, "every wisp, every wrinkle, is verily a sand-heap of stars." This discovery has been considered by some as satisfactory evidence that all nebulæ are resolvable. The sublime conception of Laplace, it has been dogmatically affirmed, can hereafter be regarded as but a "splendid vision." We reply that the *principal* evidence in favor of the theory, is that afforded by the phenomena of the solar system itself, and that *this* would not be in the least invalidated should every nebula within the reach of the telescope be resolved into stars. But no results hitherto obtained justify the conclusion that all nebulæ are composed of stars. A large proportion remain still unresolved, even under the highest power of

* This idea has also been suggested by an anonymous writer in the Westminster Review.

the "Parsonstown Leviathan." As this, however, is a point much insisted on by the opponents of the nebular cosmogony, we shall endeavor to place it in its proper light.

The hypothesis that certain nebulæ consist of cosmical vapor gradually condensing around stellar nuclei, was proposed by Sir William Herschel; that of the genesis of the solar system from a primitive nebula was suggested by Laplace. The former resulted from a critical examination of the nebulæ themselves; the latter, from a philosophical discussion of *the phenomena of the solar system.* Laplace, it is true, adopted the Herschelian theory of nebular matter, and regarded it as highly favorable to his own hypothesis. Popular writers of the present day, however, look upon the former as constituting the entire ground of Laplace's speculations. "The theory," says a distinguished author,* "proceeds on the assumption of the existence and wide diffusion of a nebulous fluid." The achievements of Lord Rosse's telescope are accordingly claimed as almost decisive evidence against Laplace's cosmogony.

Now if the mere fact that we have no certain knowledge of the *present* existence of nebular vapor warrants the inference that the *primitive* condition of the solar system was not gaseous, by parity of reasoning it would follow that unless some of the *members* of the solar system are *now* in a fluid state, our own globe has always been *solid.* But the *form* of the earth proves its ancient fluidity; and so, in like manner, various phenomena of the planetary system indicate a primitive nebular state.

When it is remembered, moreover, that the analogy of the solar system is decidedly against the assumption that the different appearances of the stars are owing to a gradation of distances; that portions of our own sideral cluster, the milky-way, *have never been resolved;* and that while Lord Rosse's telescope has separated particular *parts* of some nebulæ into stellar points, it has left other parts *unresolved*, and revealed very faint nebulous ramifications which *cannot* be composed of stars, unless we regard the components as extremely minute, the nebular hypothesis does not appear to have been materially weakened by Lord Rosse's discoveries.†

Finally, we may remark that comets and the zodiacal light *demonstrate* the existence of cosmical vapor similar to that which the theory assumes.

* James Buchanan, D.D., LL.D.

† "Regarding the *nebulæ* as spherical in form, and not as vastly long vistas foreshortened by having their ends turned towards the earth—which would be improbable, seeing there are two of them close together—the brightness of objects in their nearer portions cannot be much exaggerated, nor those in its remote, much enfeebled by difference of distance. It must, therefore, be an admitted fact that stars of the seventh and eighth magnitudes and irresolvable nebulæ may coëxist within limits of distance comparatively small, and that all inferences in regard to relative distance drawn from relative magnitudes must be received with caution."—Bartlett's Spherical Astronomy, p. 222.

3. *The alleged atheistic tendency of the Nebular Hypothesis.*—The most prominent objection to the nebular hypothesis is its alleged *atheistic tendency.* It has been characterized as an attempt to exclude God from his own universe, by substituting *natural law* for his direct agency in the work of creation. The force of this objection must evidently depend upon the question—What are we to understand by the "laws of nature?" As the question is one in regard to which much misapprehension appears to have existed, its brief consideration may not be out of place.

In the nebular theory, the process of condensation, the separation of rings and their conversion into spheroids, the acceleration of rotary velocity, solidification, &c., are supposed to have occurred in accordance with the known laws of gravitation, centrifugal force, cohesion, and chemical combination. The hypothesis does *not* assume, however, that these laws were self-originated and independent material forces. It has no conflict with Divine revelation. It assumes the existence of chaotic matter—has nothing to do with its origination. Its advocates therefore can consistently grant that matter was created by a Being of infinite power, and that the *quantities* of the various elements were determined, their collocations arranged, and their respective properties appointed by a Being of infinite wisdom. *Laws of nature are formal expressions of the ordinary modes in which the Divine will is manifested in natural phenomena.* McCosh* specifies three different senses in which the phrase is understood, but they are all embraced in the preceding definition. "A law never acts;" but it invariably points to an intelligent agent or designer. Gravitation, for instance, is neither an essentially inherent property of matter, nor an absolute *cause* of motion. The Newtonian law by which it is expressed, so far from being independent of an All-wise and Omnipotent Author, is simply the rule by which the Creator governs the material universe. The assertion therefore that this law "is probably the only efficient principle of the creation of the physical world, as it is of its preservation,"† is obviously absurd.

The fact then of the existence of a *law*, necessarily implies the existence of a *law-giver;* hence the objection which we have stated is destitute of foundation. It must follow also as a necessary consequence that the nebular hypothesis is utterly *incompatible* with the very system to the support of which a false philosophy has attempted to pervert it. Moreover, if the power of the Deity is manifested in accordance with a uniform system—a "law of nature"—in *sustaining* and *governing* the material universe, why should it be regarded as derogating from his perfections to suppose the same power to have been exerted in a similar way in the process of its formation?

* On the Divine Government, B. II., chap. i, sec. 1.
† Pontécoulant.

But some writers, among whom we may mention in particular the author of the "Vestiges," have attempted to connect the nebular hypothesis with the Lamarckian theory of development. Each of these hypotheses, however, is complete in itself. The arguments which lie against the latter have no logical bearing whatever against the former. It is not necessary to our purpose therefore to discuss the Lamarckian theory; this has been ably done by Miller, in his "Footprints of the Creator;" Buchanan, in his "Modern Atheism;" and by other distinguished writers on both sides of the Atlantic. We have no hesitation, however, in affirming that its complete refutation leaves the nebular hypothesis untouched, and that the demonstration of the latter would afford no evidence of the truth of the former. To regard them as interlinked, dependent, and essential parts of a great atheistic scheme, is to mistake entirely their mutual relationship.

The foregoing are, we believe, the most weighty objections that have been urged against the nebular hypothesis. The first, or that derived from the retrograde motions of the satellites of Uranus, has doubtless the greatest force; the most eminent astronomers, however, have not deemed it insuperable.

But if we admit, in general, the truth of the nebular hypothesis, the question still remains whether the special form of it proposed by Laplace is that which obtained in the formation of our system, or whether certain modifications ought not to be admitted. Did the chaotic matter, for instance, advance with regularity through all the stages of condensation, or were its contractions sudden and violent? The former supposition, which is that of Laplace, has been generally adopted by writers on the subject; but the latter, as has been remarked by Professor Nichol, seems more in harmony with the known operations of nature around us. When gases pass to the liquid form it is not generally by gradual condensation: on the other hand, such changes are characterized by rapid and energetic action. The same is true, at least in some cases, when bodies pass from a liquid to a solid state.

It will be seen, we think, from the foregoing discussion, that the confirmation of the nebular hypothesis would tend to reduce the apparently isolated phenomena of the solar system to the domain of law, and show that, in a cosmical point of view, the material world, in each of its diversified operations, exhibits a *uniformity of action*, adapted to develop our intellectual powers, and direct our thoughts to the great First Cause—the ever-present, all-sustaining Governor of the Universe.

Bloomington, Indiana, 1860.

ART. XVI.—*On a new Theory of Light, proposed by John Smith, M.A.;* by OGDEN N. ROOD, Professor of Chemistry in Troy University.

SEVERAL months ago, when attempting, by means of a revolving disc, to measure the time occupied by the explosion of small charges of gunpowder, the following observation was made: The flame of a burning-fluid lamp was viewed through a rotating disc provided with four radial slits, and it was found that a certain rapidity of rotation caused the lower part of the flame to assume a green hue, while by a diminished rate the whole flame was colored deep purplish red; a lower rate gave a violet tint, alternating with pure white.

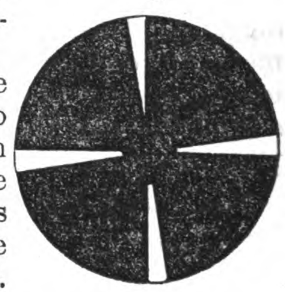
It was evident that these appearances depended much on the state of the eye, for they often could be perceived only after it had become a little fatigued by the blinding effect consequent on the comparatively slow succession of the impressions of light; in addition to this, I found that my colleague, Prof. Vincent, though recognizing the green tint, was unable to perceive the red hue, with which my eyes at that very moment were dazzled.

I considered these appearances, therefore, as *subjective*, and laid the matter aside for future experiment. As, however, similar phenomena have been observed in England, and have been thought to have not only an objective existence, but to be worthy materials on which to build a new theory of light, it may not be amiss to enter into a slight examination of their nature.

In Mr. Smith's experiments, of which I have been able to obtain only the account given in the March number of this Journal, bright white light is allowed to act on the eye during a certain fraction of a second; it is succeeded by shadow or darkness, which lasts also during a certain short interval of time, when the operation is repeated anew, &c.

This pulsation of light and shade the author effects in a variety of ways: the result is color—a yellowish green, purple, pink, &c. Fechner, to whom we are indebted for extensive researches on sight, several years ago observed that white discs having black spiral figures painted on them, when set in rotation exhibited colors which he considered subjective.*

1.



That these colors are really subjective, the following simple experiment may serve to show: A blackened disc nine inches in diameter, was cut with four slits of the shape seen in the wood cut; the width of the slits at the circumference was $\frac{5}{10}$ of an inch; the disc was made to rotate before a bright cloud.

* Pogg. Ann. vol. xlv, p. 227.

A rate of ten revolutions per second caused the cloud in a short time to appear of a deep red color, having in it a tinge of purple, or, according to Mr. Smith, the disc transmitted pink light; it was now viewed through a plate of orange-tinted glass. Previous experiment with a small telescope provided with a micrometer, and a flint glass prism, had shown that this orange colored glass readily transmitted the red, orange, yellow, and a portion of the green, but that it was opaque to the blue and violet rays; it was therefore fairly to be expected that if the disc was really transmitting red light, the plate of glass would do the same. The result was different; through the glass the disc appeared of a bright greenish-blue color. This experiment is very easy to make, and the effect is brilliant.

Plates of glass of other tints were now employed; the results are given below.

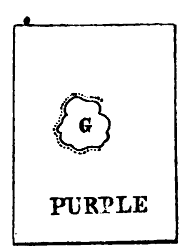
Medium.	*Tints of the Disc*
Yellow glass,	Violet,
Green "	Green, neutral, or faint red,
Red "	Red, neutral, or faint green.

As the green glass was nearly opaque to red rays, the effect of its use ought to have been darkness. It is evident by an inspection of this table that the disc really transmitted *white*, and not red light, which becoming colored by its passage through the plates of glass, induced in the retina, from time to time, the sensation of the complementary tint, more or less mingled with the *original* impression.

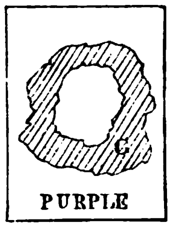
Having now shown that, contrary to Mr. Smith's supposition, the light transmitted by the revolving disc is really white, let us notice some of its effects on the eye.

For this purpose I caused perforated discs to revolve at uniform rates by means of clock-work; the arrangement being similar to that employed by Plateau. A blackened disc five inches in diameter and perforated with four slits 7° 12′ in the width was set in rotation, and the bright sky viewed through it; the eye of the observer being immediately behind the disc. With a rate of 11½ revolutions per second, the appearance of the window was as in fig. 2; a central spot was colored bluish-green, the rest of the field was purple, or reddish purple, according to the state of the eye. The green spot remained always in the axis of vision, and moved with each change of it. With the exception of fluctuations in the outline of the spot, this appearance remained tolerably constant, as long as the rate of revolution continued the same. The spot or shadow was fringed with a narrow, faint blue border, indicated by the dotted line.

2.



8.



Upon increasing the rate of revolution, the bluish-green spot expanded into an irregularly shaped ring, and continued to expand, filling the field, till the rate had become as high as 15 rotations per second, when often the field for an instant became of a greenish tint which was succeeded by a bluish tint; upon increasing the speed this also vanished. Still higher rates cease to produce any of these peculiar effects on the eye.

Upon slowly reducing the rate to 9 revolutions per second, the green spot contracted in dimensions, and assumed a yellow tint, while the field often became at the same time tinted deep crimson. With a rate still lower, the appearance of the field is variable and the tints flickering; it assumes sometimes a purple, a yellow, or a yellowish green tint.

This experiment I repeated a great number of times, with the same general result, and though it sometimes happened that the eye became insensible to these colors, from repetition, momentary rest in darkness restored this power for a short time. Thus it occurred that the tints were sometimes seen with great distinctness, while at others they could hardly be distinguished.

Upon a dark cloudy day to produce these effects it was found necessary to increase the width of the slits to 20°; from whence it was manifest that lack of intensity in the light might be made up by its longer duration.

It would appear then, from these experiments, that light from a bright cloud, if allowed to act on the eye repeatedly during from $\frac{1}{450}$ to $\frac{1}{150}$ of a second, developes subjective colors; that, however, the development of the subjective tint is dependent not so much on the length of time which the eye is exposed, as upon the interval of *rest* or shade which follows each exposure, may be shown in the following manner: In the experiment where with $11\frac{1}{2}$ revolutions a reddish purple was produced, the exposure lasted $\frac{1}{575}$ of a second; the interval of rest or shade was $\frac{1}{50}$ of a second; now a disc was cut similar to fig. 1, but having eight slits, each 7° 12′ in width, when it was found that 5·5 instead of 11·5 revolutions per second produced the purplish-red tint; here the exposure was twice as long, but the interval of rest or shade nearly the same. With sixteen slits, $2\frac{8}{10}$ revolutions produced the same tint, the exposure being of course four times as long, but the interval of rest nearly the same. Determinations of the length of this interval are given below:—

Length of the intervals of shade required for the production of

Blue, ... $\frac{1}{65}$ sec.
Purplish-red, ... $\frac{1}{50}$ "

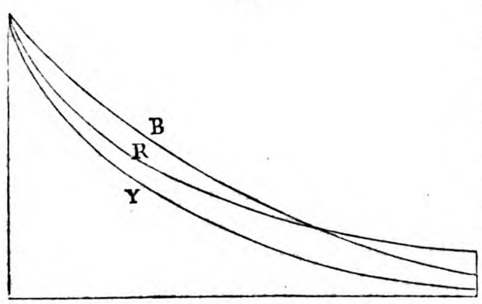
To ascertain exactly what portions of these intervals elapse

before these tints make their appearance, or how long the tints are actually seen by the eye, is another matter, and would no doubt require an elaborate series of experiments, though it would seem probable that at least half of the time of the above given intervals passes before the subjective color makes its appearance.

The table does not apply to the axial portion of the retina, which is almost always differently affected. That the change in susceptibility in the retina is progressive outwards, is shown by the gradual expansion of the green ring; that it varies from second to second, is seen in the fluctuations of the outline of this ring.

The occurrence and sequence of these subjective colors may easily be explained by supposing that during the interval of rest or shadow the action of the yellow rays diminishes more rapidly than that of the red, the red more rapidly again than that of the blue. If this takes place as indicated by the curves below, it is easy to understand the production of the tints, for if the moment after the blue has been developed white light be again presented to the eye, it is evident that neither purple nor red will be seen, sufficient time not having been allowed for their production.

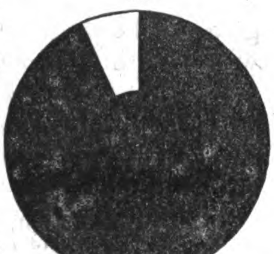
4.



The same reasoning applies to the axial portion of the retina, which, owing to its greater sensibility to such impressions of light, requires a somewhat longer interval of rest before the reaction occurs.

If the impression be too strong, that is, if the light be too bright or the eye too long exposed to it, these peculiar effects are not observed, and during such short intervals of rest as $\frac{1}{50}$ or $\frac{1}{60}$ of a second the white clouds seen through the discs suffer no change in tint; but if a blackened disc twelve inches in diameter be cut as seen in the figure, with an aperture of 30°, and made to rotate before a white cloud at a rate of only one revolution per second, the eye placed as near it as possible, will most distinctly see, in the interval of darkness, an image of the sky, of a bluish-green tint. Whence it follows that an exposure of the eye to white light lasting $\frac{1}{12}$ of a second, induces in it for a considerably longer time the sensation of this color.

5.



In general these experiments seem to point out that after momentary exposure to white light subjective colors are induced in the eye, whose tint and duration are dependent on the strength of the impression received, as well as upon the length of time allowed for rest; these sensations of color apparently having a relation to the colors observed after looking at the sun, similar to that which a temporary disorder bears to a chronic affection.

Troy University, March 20, 1860.

ART. XVII.—*On the Meteor of November* 15th, 1859; by Prof. H. A. NEWTON, Yale College.

IN the forenoon of November 15th, 1859, about half-past nine o'clock, a meteor or meteorite was seen to descend towards the earth by many persons at widely different places. Throughout the southern part of New Jersey a tremendous explosion was heard immediately after its appearance. Mr. Benjamin V. Marsh of Philadelphia has published in the Journal of the Franklin Institute, a very valuable collection of statements of persons who saw it from various positions, from Newburyport, Mass., to Petersburg, Va. In the January and March numbers of this Journal Prof. Loomis has also given a selection from the accounts of several persons who were so fortunate as to witness it.

The physical circumstances attending its passage through the atmosphere, its explosion, and its entire or partial combustion are of interest and the accounts referred to are of great value in studying them. My present purpose, however, is to determine as accurately as possible the *path* and *velocity* of the meteorite. The result of my investigation has been to establish almost beyond a doubt the conclusion, that *this body was not a member of the solar system but came to us from the stellar regions.*

In several instances have the paths of meteors or shooting stars been determined and velocities been computed which would necessitate with respect to them the same conclusion. But in general the data are so vague that the result lacks the certainty which is afforded by the meteor of November. This is especially true of the velocities given by M. Petit of Toulouse. In not a single instance have the results given by him seemed to me worthy of confidence. I have thought, therefore, that a careful discussion of this meteor was desirable.

The observations from Alexandria, Va., and New Haven, Ct., seem for several reasons the best for determining its path through the atmosphere. Mr. Marsh quotes the following from a letter of Mr. Caleb S. Hallowell of Alexandria.

"Abram Martin, a student particularly well qualified for such observations, was fortunately, standing perfectly still with the

meteorite in full view during its entire visibility, and he seems to have completely daguereotyped its path and appearance on his mind. By my direction he made at the time a careful note of the spot where it fell, and as to the point in the heavens from whence it originated, he, on two *independent* and *distinct* occasions pointed the telescope of the theodolite very nearly to the same spot, as given below.

	1st obs'n.	2nd obs'n.	Mean.
Apparent altitude at moment of appearance, .	40°	$39\frac{1}{2}$°	$39\frac{3}{4}$°
Azimuth, " " "	N. 83 E.	N. 85 E.	N. 84 E.
Apparent altitude at moment of disappearance, behind houses, ...	11°	10°	$10\frac{1}{2}$°
Azimuth, " " "	N. 77 E.	N. 76 E.	N. $76\frac{1}{2}$ E.

The time of flight was estimated at two seconds. The determination of the point from which it originated was made some weeks after the 15th of November.

In New Haven it was seen by Judge W. W. Boardman, who was standing at the time in the west door of the Exchange Building, Church Street. He was conversing with a friend, and looking S.E., when his attention was caught by the meteor down the street.

It shot obliquely down across the street and disappeared behind the cupola of the Catholic church, which is 700 or 800 feet distant. The course of the street is about S. 29° W. He says it must have been seen as far east as the line of the street. Mr. E. C. Herrick, from conversation with Judge Boardman some days after the meteor was seen, concluded that the most probable angle of its path with the vertical was 33°, though it might vary from this several degrees. This inclination was obtained, first by calculation from the apparent altitude at the moment of appearance, and second, by holding up a rod at the inclination which Judge Boardman said equalled that of the meteor's path and measuring this inclination. The perpendicular lines of the buildings would help to impress on his mind the direction. The point of disappearance was, in azimuth S. 35° 34′ W., at an altitude of about 6°. The time of flight was estimated as one second, or between one and two seconds.

The position of Judge Boardman was N. lat. 41° 18′ 18″, W. long. 72° 55′ 10″. That of Alexandria is N. lat. 38° 49′, W. long. 77° 4′. There are indications which go to show that the path of the meteor was not a straight line, especially near its lower extremity. But the part seen by Judge Boardman must have been very nearly straight, and may be so considered without serious error. The plane passing through New Haven, and the point of the heavens S. 35° 34′ W., altitude 6°, and making there an angle of 33° with the vertical, meets the plane passing through Alexandria and the two points given in the last column of the table of Mr. Hallowell in a straight line, which I shall consider the path of the meteor. This line cuts the earth

in W. long. 75° 10′ 30″, and N. lat. 39° 13′, a place a mile north of False Egg Island Point, just on the eastern shore of Delaware Bay. The azimuth of the point from which the meteor would approach the earth, at that place, was N. 88° 55′ E., and the altitude was 49° 35′.

Observations at New York, and also in the neighborhood of Dennisville, N. J., prove that the azimuths of the points of disappearance at each station are quite exact. The only important question is, whether the inclinations reported are reliable. To test their general accuracy, I have collected such other observations as seem at all reliable, and can be compared with them.

The inclination of 33°, at New Haven, is confirmed by observers at Salem, Mass., Waterbury, Ct., New York city and Newark, N. J. These places were all N.E. of the meteor and nearly in the same azimuth as New Haven. In the third column of the table below, is given the reported inclination, and in the fourth, the angle at which the path above assigned to the meteor would be seen to be inclined to the vertical circle at the horizon.

	Place	Observed inclin.	Calculated inclination	Obs. – cal.	Cal. – obs.	Observers.
1	Salem,	40°	31° 21′	8° 39′		Francis F. Wallis.
2	Waterbury,	30	34		4°	Wilder Smith.
3	New York,	10	35 27		25 27′	Mr. Gould.
4	" "	20	35 27		15 27	Mr. Latham.
5	" "	35	35 27		27	J. P. Pirrson.
6	" "	45	35 27	9 33		Mr. Bradley,
7	Newark,	45	36 33	8 27		Henry J. Mills.

The observations that appear to me from the circumstances of the observer most worthy of confidence next to that at New Haven are taken in order, the fifth, the second, and the seventh. These in fact agree best with the New Haven inclination. The third I feel disposed to leave out of consideration. Perhaps the fourth should be likewise discarded. The numbers in the fourth column, refer to the horizon, the third in general to points above it. They are not therefore exactly comparable. The numbers in the fifth and sixth columns appear large, but if we consider only those observations that appear worthy of confidence, I think the accordance is as great as can usually be expected for a daylight meteor. The probable error would be quite small. The errors are pretty nearly the same in excess as in deficiency. The assumed angle of inclination therefore cannot be far from the truth.

The inclination of the meteor's path as actually seen at Alexandria, must have been nearly the same as that resulting from the mean of the observations reported by Mr. Hallowell. The arc passing through the two given positions is at the horizon inclined 11° 34′ to the vertical. The meteor was seen by three persons at Washington, two of whom say it descended vertically.

The third says it apparently descended to the ground a little north of the point of its first appearance.

The computed trajectory would be seen at Washington inclined 9° 30′ to the vertical circle.

Mr. Nickerson, four miles west of Dover, says the column of smoke was nearly or quite vertical.

Mrs. Cowgill, six or seven miles west of Dover, says that if it varied at all from the vertical, it disappeared to the north of the point where it was first seen.

The inclinations at these two places of the computed trajectory, and the vertical, are 7° and 6½° respectively.

A line but little inclined to the vertical would be easily considered strictly vertical. But one would not readily mistake the direction of inclination. For my present purpose, moreover, an error of several degrees in the inclination at Alexandria would be of little importance.

The length of the visible track of the meteor is the next thing to be computed. Judge Boardman is confident that he saw it as far east as the line of Church street. It was probably visible sometime before he saw it. If S. 29° W. be assumed as the direction in which it was first seen we have a visible arc of about 12°, corresponding on the real line of the meteor to a distance of 36, or more exactly 35·91 miles. The motion was nearly at right angles to the line of vision. One extremity of this line was in N. lat. 39° 13′ 33″, W. long. 74° 24′ at an altitude of 49¼ miles. The other extremity was in N. lat. 39° 13′ 15″, and W. long. 74° 49′ 40″, at an altitude of about 22 miles. After the meteor disappeared to Judge Boardman, it must have passed several miles before it exploded and vanished. Many accounts agree in this, that it disappeared above the horizon. Some say it burst into fragments, each leaving behind a train of light. At Tuckerton, N. J., it disappeared at 10° altitude. Other accounts make me think this not far from the truth. This would make it explode at an altitude of a little less than 2°, as seen from New Haven. It would add 5° to the 12° of the visible arc and about 14 miles to the track of the meteor, making it in all, about 50 miles long from the point assumed, as that first seen by Judge Boardman.

Prof. Loomis, from the reports of several persons who saw the meteor in New York city, says, "the length of its visible path, was variously estimated from 15° to 25°. The entire period of its visibility did not exceed one or two seconds." By most of these observers, both the upper and lower portions of the meteor's path were probably not seen. At first it would not attract attention and it usually disappeared behind buildings. They probably saw in the average nearly the same amount of the meteor's path as Judge Boardman. This at New York city, subtends an arc of about 18°.

The meteor was no doubt seen at Alexandria much sooner than at New Haven. I am inclined to think that the reported altitude of its first appearance is less correct than the inclination. Taken strictly, the visible arc corresponds to a distance of 522 miles on the meteor's path. But a change of one degree in the place of first appearance might diminish this nearly 80 miles. When first seen at New Haven, it had an altitude of 17° 25′ at Alexandria. If the altitudes had been estimated, and not measured, the 17° 25′ might be considered 40°. But the method employed, that of directing an instrument to that point in the heavens, seems well fitted to give tolerably correct results. Though we cannot determine the distance passed over by the meteor while in view, yet it is evident that Mr. Martin saw it much sooner than Judge Boardman. If we were to allow an error of 15° in the place of first appearance, the length of the visible path of the meteor would be at the least 90·8 miles. If in addition, an error of 5° be allowed in the direction of the meteor's path, the length would not be less than 70 miles. If even the meteor's path was 10°, and the place of first appearance 15° in error, the visible path would be 57 miles.

These last errors seem very much greater than can be allowed. Mr. Hallowell says, "I cannot believe the body could possibly have had a less altitude than 35° at the time it was first seen." Ten degrees change in the direction of the meteor's path would carry it about 8° towards the vertical as seen from New Haven and New York city, a change which I think the observations would hardly allow.

The Washington observers, to some extent confirm the Alexandria observation. One says it appeared first at an elevation of 50°. Dr. Mackie says that the meteor had a luminous train extending vertically 15° to 20°. When first seen, its base was about 30° from the horizon. The point where it was first seen, by Judge Boardman, would have, at Washington, an altitude of 17° 40′.

The *time* of *flight* is the most difficult element to determine. My purpose is not to compute the actual velocity. I wish rather to prove that it was much more than 21 miles per second, when the body entered the atmosphere.

Judge Boardman estimates the time at one second, and says that it could not have been as great as two seconds. He estimated it, by supposing a body to pass with the same velocity over a similar distance, and noting the interval. The person to whom he was speaking, was not able to get a view of the meteor. The most probable velocity from this observation, would be 36 miles: the least velocity over 18 miles a second. The New York city observers, reported by Prof. Loomis, saw about the same amount of the meteor's path, as Judge Boardman. "The entire period of visibility did not exceed one or two seconds." The velocity then

would be not less than 18 miles a second, and probably it was much greater.

Mr. Mills estimated the time at two seconds. The arc passed over seems to have been 15° or 20°. This would give a velocity of about 18 miles a second, if his estimate of time is correct.

Dr. Mackie of Washington, says: "It was perhaps two seconds in view, for I had time, after seeing it first, to grasp my companion's arm, and point to it, before it disappeared." The altitude of its first appearance being estimated, and not measured, the velocity cannot be easily determined from this observation. But considering how liable an observer is to over-estimate the time of flight, I think the velocity, so far as indicated by his observation, is *much greater* than 20 miles a second.

The time of flight at Alexandria was estimated nearly in the same manner as at New Haven. The velocity which is indicated by this observation depends on the amount of error we can allow in the determination of the point of first appearance. Taken strictly, we have a velocity of 260 miles a second. Though I should not be unwilling to admit such a velocity, if we had valid proof of it, yet the present observation cannot be considered as furnishing it. Allowing a possible error as great as mentioned (p. 190) we have a velocity of 45, or 35, or 28½ miles per second. Only one person, that I am aware of, gives a period of time exceeding two seconds. Mr. Wallis of Salem, Mass., says it was in sight from five to eight seconds.

Besides these specific estimates of time, we have other reasons of greater or less weight for calling it very short.

Mr. Marsh in his paper argues with great reason, that "the extreme shortness of the time occupied in its flight is proved, not merely by the estimates of several observers, but by the failure of people in the vicinity of the explosion to distinguish the source of the sudden flash of light seen by them, and by the impression of even the most distant observers, that it fell very near to them." The latter reason, especially, has much weight.

The light is always called a "flash of light," by some a *sudden* or *instantaneous* flash.

A large number of observers state that they were unable to call the attention of those standing by them to the meteor. It seems that only those looking towards that part of the heavens, saw it.

In a letter dated June 13th, Mr. Marsh says, "all I have since heard from parties I have conversed with tends to confirm the shortest estimates, the impression generally being that it was instantaneous or nearly so."

In reasoning from these data, two considerations should be kept in mind.

1st. The natural tendency is to make the time of flight too great, and hence the velocity, too small.

2nd. From the moment the meteor entered the atmosphere, it would lose velocity. The resistance which the air offers to so rapid a motion, is enormous. If meteorites be admitted to come in general from meteors, it may be added that they rarely enter the ground more than two or three feet. They do not strike the earth with a velocity at all comparable to that which meteors are known to have, in the higher regions. They lose almost all their velocity in passing through the atmosphere.

A careful examination of all these observations leads me to believe that the actual velocity was as great as 36 miles a second. If we consider the resistance of the air, and then make as large an allowance for errors of observation as can reasonably be made, it seems almost impossible that it could have entered the atmosphere with a velocity less than twenty-one miles. The parts of the earth directly under the meteor, were by the earth's motion in its orbit, and on its axis, moving in a line inclined 89° 31′ to the path of the meteor, with the velocity of 19·023 miles. If the velocity of the meteor in this path was 21, its velocity relative to the sun would then be a little more than 28½ miles. If the meteor had been moving in a parabolic orbit around the sun, it would have had from the combined action of the earth and sun, a velocity of 27·9 miles a second. If, therefore, as I think, can hardly be doubted, the meteor entered the atmosphere with a velocity not less than 21 miles, *it must have been moving in a hyperbolic orbit.*

We have been accustomed to consider the solar system as filled with small planetoids, millions of which, each day, come into the atmosphere, and are burnt up, causing the *shooting stars.* Now we find that we must, in all probability, add one, and no doubt innumerable other similar bodies to the stellar spaces. It opens a new view of creation.

It must not hence be imagined, that the meteors and shooting stars all come from the stellar spaces. The periodicity of the August and November meteors, shows plainly that they are from permanent members of the solar system.

This meteorite did not come from the moon. If we could suppose a lunar volcano to throw out a body with such an enormous velocity, that body must come to the earth, nearly from the direction of the moon. But the moon was at that time about 120° from the direction of the meteor's path.

The recent researches, respecting the transformation of motion into heat, throw some light on the subject of shooting stars. When these bodies come into the atmosphere, the motion they lose is transformed into motion of the air, heat, light, sound, and probably other forms of energy. If it was all transformed into heat, it would be easy to compute the amount due to the loss of a given velocity. If they have a motion of their own, and their directions are subject to no law, it is easily seen that the average

velocity is much greater than 19 miles a second. A body weighing one pound, and moving 25 miles a second, has momentum sufficient to raise $(25\times5280)^2\div2g=271{,}500{,}000$ pounds one foot. By Joule's equivalent the raising of 772 pounds one foot, corresponds to the heat necessary to raise one pound of water one degree Fahrenheit. If the capacity of the meteoric substance for heat is 0·2, (that of iron is 0·12,) the loss of a velocity of 25 miles would be equivalent to heating $(271{,}500{,}000\div0{\cdot}2)\div772=$ 1,760,000 pounds of the substance one degree Fahrenheit, if the whole of the motion was transformed into heat. A very small fraction of this heat would doubtless suffice to burn up, or dissipate, any substance whatever.

It is often urged, that the shooting stars cannot be solid bodies, since of the millions that daily enter the atmosphere, so few come to the ground. The above calculation shows that the heat generated may be ample to vaporize or dissipate them.

The shooting stars need not in general be large bodies. The apparent size is due to irradiation, and indicates, not amount of matter, but rather amount and intensity of light. Thus the stars though often spoken of as mere points have disks. The diameter of stars of the first magnitude was estimated at 2′ by Tycho Brahe. The telescope has shown that this disk is spurious. If these stars are equal in size to the sun, Tycho's estimate makes their diameters 50,000 times too great.

It has been estimated that the light of the sun's surface is four or five times as great as that of the same surface of the lime in the calcium light. It is also estimated that the light of the sun is 20,000,000,000 times that of Sirius. A simple calculation shows that an inch globe as brilliant as the calcium light, would give at over 100 miles distance as brilliant a light as a star of the first magnitude. The estimates which are used as the basis of calculation are confessedly very vague, yet they show that a very small body may furnish as much light as a shooting star. Such a body would naturally burn up without passing through the atmosphere.

I can therefore see no reason, as some persons do, to make a marked distinction between the different classes of meteors. Those which furnish meteorites, those which explode with a loud report, and those of all degrees of brilliancy which are not heard to explode, all seem to belong to one class, and to differ from each other no more than substances on the earth. That some are solid and others aëriform is not impossible. Differences of chemical constitution, size, velocity, and orbit exist, and these may account for the variety of appearance.

Note.—Since the above was in type, the meteor of July 20 seems to furnish better data for proving that meteors sometimes come from the stellar spaces.

ART. XVIII.—*Crystalline form not necessarily an indication of definite Chemical Composition: or, on the possible variation of constitution in a mineral species independent of the Phenomena of Isomorphism.* By JOSIAH P. COOKE, Jr., A.A.S., Professor of Chemistry and Mineralogy in Harvard College.*

IN a memoir presented to the American Academy of Arts and Sciences in September, 1855,† I described two new compounds of zinc and antimony which I named *stibiobizincyle* and *stibiotrizincyle*, on account of their analogy in composition to the metallic radicals of organic chemistry. The symbols of these compounds are $Sb\,Zn^2$ and $Sb\,Zn^3$; and they are distinguished by the high perfection of their crystalline forms, the last being still further characterized by a most remarkable property of decomposing water quite rapidly at 100° C. I stated in the same memoir that crystals of these two compounds could be obtained containing proportions of zinc and antimony differing very widely from those required by the law of definite proportions; and I also traced out the relation between the composition of the crystals, and that of the menstruum in which they are formed. It is my object in the present paper to consider the bearing of these facts, already fully described, on the idea of mineral species, and to offer a few suggestions which I hope may be of service in determining the true chemical formulæ of many minerals, and thus in simplifying the science of mineralogy. But in order to render myself intelligible, it will be necessary to recapitulate very briefly the facts in question, referring to the original memoir for the full details.

The crystals both $Sb\,Zn^2$ and $Sb\,Zn^3$ can be obtained with great readiness. It is only necessary to melt together the two metals in the atomic proportions, and when the metals are fully alloyed, to proceed exactly as in crystallizing sulphur. The melted mass is allowed to cool until a crust forms on the surface, which then is broken, and the liquid metal remaining in the interior poured out. On subsequently breaking the crucible, the interior is found lined with magnificent metallic crystals, which, when not tarnished by oxydation have a silver-white lustre. In the course of my investigations on these compounds, crystallizations were made, or attempted, of alloys, differing in composition by one half to five per cent, according to circumstances, from the alloy containing 95 per cent of zinc, to that containing 95 per cent of antimony; *but only two crystalline forms were observed*, that of $Sb\,Zn^2$ and that of $Sb\,Zn^3$. The crystals of the

* Communicated by the Author.

† Transactions of the American Academy of Arts and Sciences, New Series, vol. v, p. 337. This Jour. [2], xx, 222.

two compounds both belong to the trimetric system; but they differ from each other, not only in their crystallographic elements, but also in their whole "habitus." Stibiotrizincyle crystallizes in long acicular prisms, which group themselves together into larger prismatic aggregates; while stibiobizincyle crystallizes in broad plates, which twin together on an octahedral face, and form a very characteristic cellular structure. This very striking difference in the character of the crystals proved to be an important circumstance in the investigation, as it enabled me to distinguish with certainty between the two compounds, even when the faces of the crystals were so imperfect that a measurement of angles was impossible.

The most remarkable result of the investigation, and the one to which I wish to direct especial attention, is the fact that each of the two crystalline forms was found to be constant under very wide variations in the per-centage composition of the crystals. As this is a point of great importance, it will be necessary to enter more into detail, considering in the first place the crystals of $Sb\,Zn^3$. The crystals of this compound are obtained in the greatest perfection from an alloy containing the two metals in just the proportions represented by the formula, namely, 42·8 parts of zinc, and 57·2 parts of antimony. They are then comparatively large, generally aggregated, and, as the three analyses cited in the accompanying Table indicate, they have the same composition as the alloy.

Composition of the alloy by synthesis.		Composition of the crystals by analysis.		
Per cent of Zn.	Per cent of Sb.	Per cent of Zn.	Per cent of Sb.	Sum.
42·80	58·20	43·15	56·93	100·08
"	"	43·06	56·50	99·56
"	"	42·83	57·24	100·07

On increasing gradually the amount of zinc in the alloy up to 48·7, the crystals continued to have the composition of the alloy; and the only difference which could be observed in their character was that they were smaller, and more frequently isolated. Between these limits the whole mass of the alloy exhibited a strong tendency to crystallization; and by pouring it, as it cooled, from one vessel to another, it could be crystallized to the last drop. On increasing the amount of zinc in the alloy to 50·7 per cent, the amount of zinc found in the crystals was uniformly less than it was in the alloy; but no closer relation between the two could be detected, owing, undoubtedly, to the unavoidable irregularity in the crystallization of the alloys which contained more than 50 per cent of zinc. This arose from a peculiar pasty condition which the liquid mass assumed at the point of crystallization. Definite crystals, however, were obtained from an alloy of 60 per cent zinc containing 55 per cent; above this the crys-

tals became less and less abundant, and gradually faded out, although the alloy of 86 per cent of zinc exhibited a radiated crystalline texture; and a trace of this structure could still be discovered even in the alloy containing only 4 per cent of antimony. It was very interesting to trace the gradual fading out of the crystalline structure, as the character of the phenomenon was entirely analogous to that which may be noticed in many crystalline rocks.

Finding that the crystalline form of $Sb\,Zn^3$ was constant under so great an increase of the proportion of zinc in the crystals, it might be supposed that, on returning to the alloy of 42·8 per cent of zinc and increasing the amount of antimony, we should obtain crystals containing an excess of antimony; but so far is this from being true, that the slightest excess of antimony entirely changes the character of the crystallization. On crystallizing an alloy containing 41·8 per cent of zinc, not a trace of any prismatic crystals could be seen; but in their place there was found a confused mass of thin metallic scales, which, as will soon be shown, are imperfect crystals of $Sb\,Zn^2$. Thus it appears that, although perfectly formed crystals of $Sb\,Zn^3$ can be obtained containing 55 per cent of zinc (that is, 12 per cent above the typical proportions), they cannot be made to take up the slightest excess of antimony.

Let us pass now to the crystals of $Sb\,Zn^2$. In order to obtain crystals having the exact typical constitution, it was found necessary to crystallize an alloy at least as low as 31·5 per cent of zinc. At this point large compound crystals are obtained corresponding to the large crystals of $Sb\,Zn^3$; and the same was true of alloys down to 27 per cent of zinc. Between these two limits (namely, alloys of 31·5 and 27 per cent of zinc) the crystals formed were found to have the theoretical composition of $Sb\,Zn^2$, indicating of course a tendency towards this point; but on increasing or diminishing the amount of zinc in the alloy beyond these limits, the composition of the crystals immediately began to vary in the same direction as that of the alloy. The crystals of $Sb\,Zn^2$ containing an excess of zinc are smaller and more frequently isolated than those having the exact theoretical composition. A similar fact, it will be remembered, is true of the crystals of $Sb\,Zn^3$.

At the alloy of 33 per cent of zinc, the definite crystals of $Sb\,Zn^2$ begin to disappear, and are succeeded by thin metallic scales, which are obviously imperfect crystals of the same form. This was established, not only by the obvious law of continuity noticed in the different specimens (the perfect crystals gradually passing into the scales), but also by the peculiar mode of twining, which was the same with the scales as with the large crystals, forming the peculiar cellular structure already referred to. Moreover, the angle between two scales thus united was found

to be equal to the basal angle of the perfect crystals, at least as nearly as could be measured. These scales continue up to the alloy of 41·8 per cent of zinc, becoming, however, less abundant and less distinct. Several specimens of them were analyzed; but no regularity could be detected in their composition, except that they all contained a much larger amount of zinc than the alloys in which they were formed.

Crystals of $Sb\,Zn^2$ containing an excess of antimony were readily obtained from alloys containing less than 27 per cent of zinc. They became more and more imperfect as the excess of antimony increased, and finally faded out altogether in the alloys below 20 per cent of zinc. It is evident, therefore, that definite and perfect crystals of $Sb\,Zn^2$ can be obtained with a large excess either of zinc or antimony above the theoretical composition. It is also evident that, of the two compounds, $Sb\,Zn^2$ is the most stable,—first, because it is formed to the exclusion of $Sb\,Zn^3$ in all alloys containing less zinc than the amount corresponding to the typical composition of the last compound; and secondly, because the crystals retain the typical composition under quite a wide variation (viz. between 31·5 and 27 per cent) in the composition of the alloy.

The facts above stated are fully illustrated by the following Table, which gives the results of a large number of analyses of crystals of both compounds formed in alloys containing different proportions of the two metals:—

Analyses of the Crystals formed in the Alloys of Zinc and Antimony.

Stibiotrizincyle.					Stibiobizincyle.				
Composition of the alloys by synthesis.		Composition of the crystals by analysis.			Composition of the alloys by synthesis.		Composition of the crystals by analysis.		
Per cent of Zn.	Per cent of Sb.	Per cent of Zn.	Per cent of Sb	Sum.	Per cent. of Zn.	Per cent of Sb.	Per cent of Zn.	Per cent of Sb.	Sum.
70·40	29 60	64·15	35·77	99·92	33·00	67·00	35·37	64·57	99·94
66·50	33·50	61·00	39·00	*100 00	33·00	67·00	35·40	64·60	†100·00
64 50	35·50	53·50	41·44	99·94	32·50	67·50	34·62	64·92	99 54
....	....	55·49	44·42	99 91	32·50	67·50	34·61	65·39	†100·00
60·60	39·40	55·00	45 09	100·09	31·50	68·50	33·95	66 09	100·04
58·60	41·40	50 39	49·29	99·68	29 50	70·50	33 62	66·38	†100·00
56·60	43·40	49·92	50 05	99·97	29·50	70·50	33·62	66·38	†100 00
54·70	45·30	48·26	51 42	99 68	27 50	72·50	33·85	65·81	99·66
52·70	47·30	47·47	52·53	†100·00	26·50	73·50	32·08	67·60	99·68
....	....	....	....	....	26 00	74·00	30·74	69·06	99·80
50·70	49·30	46·89	53·11	†100·00	25·50	74·50	30·43	69 51	99·94
50·70	49·30	46·45	53 55	†100·00	25·00	75·00	29·88	70·20	100 08
48·70	51·30	48·66	51·34	†100·00	24·50	75·50	28·76	71·24	100·00
46·70	53·30	46·77	53·23	†100·00	23·50	76·50	27·93	71·85	99·78
44·80	55·20	44·26	55·73	†100·00	22·50	77·50	26·62	73·27	99 89
43·80	56·20	44·04	55 96	†100·00	21·50	78·50	24·83	74·74	99·57
42·80	58·20	43·15	56·93	100·08	20·12	79·88	20·58	79·42	100·00
42·80	58·20	43 06	56·50	99·56					
42·80	58·20	42·83	57·24	100·07					

* In this analysis the antimony only was determined.

† In this analysis the zinc only was determined.

The relation between the composition of the crystals $Sb\,Zn^2$ and that of the alloy in which they are formed, is discussed at length in the memoir already referred to. It is there shown to be a very simple function of the mass of metal which is in excess in the alloy, and of the force which determines the union of the elements in definite proportions. The whole order of these phenomena seem to the Author to point to the existence of a power in the mass of metal which is in excess in the alloy, to disturb the action of the force, whatever it may be, which tends to unite the elements in definite proportions. There is, in the first place, a strong tendency in the elements to unite and form crystals having the exact typical composition; and secondly, this tendency is only overcome by a certain excess of either metal in the alloy. Then, again, the crystals of one compound obviously interfere with those of the other. This certainly has the appearance of one force interfering with the action of another,—the force of mass (if I may so call it) perturbing the action of the chemical force. But it is not my object at present to enter into a discussion on the cause of this variation. Moreover, since such a discussion must be based on purely hypothetical grounds, we could not expect to arrive at any definite conclusion. The facts will be viewed differently according to the theory which may be adopted in regard to that long-controverted subject, the essential constitution of matter. Leaving, however, all theoretical considerations aside, there are certain practical bearings of the *observed facts* on the science of mineralogy which are of immediate application.

Here are two beautifully crystallized products, as well crystallized as any that occur in nature, and yet the different specimens of the crystals differ from each other so widely in composition that any single analysis might lead to an entirely erroneous conclusion in regard to the general formula of the substance. Were a chemist to analyse accidentally solely the crystals obtained from an alloy containing 58·6 per cent of zinc, he would at once determine that the formula of the compound was $Sb\,Zn^4$; and by a like accident he might be led to any other formula between this and $Sb\,Zn^3$: in fact, by an analysis of a number of specimens of needle-shaped crystals obtained from alloys of copper and tin, Rieffel was led to several just such improbable formulæ; and in my own investigations it was not until I had analysed a whole series of crystals, that the real nature of the phenomena became apparent, and the true constitution of the compounds determined. If, then, such great variations in composition are compatible with a definite crystalline form in these furnace products, may not similar variations occur in the crystalline minerals formed in nature?

It is not necessary to make an extended investigation in order

to answer this question; for the materials at our hands are sufficient to give us a satisfactory reply.

There is a compound of antimony and silver called discrasite, which occurs in many localities crystallized in trimetric prisms homœomorphous with $Sb\,Zn^3$. The formula of the mineral is therefore probably $Sb\,Ag^3$, which would require 71·5 per cent. of silver; but the per cent as given by analysis varies between 75·25 and 78 per cent, and one analysis gives the per cent as high as 85. Further analyses of this mineral are required in order to determine its constitution, but there can be no doubt that it varies in composition like $Sb\,Zn^3$.

Silver-glance is another highly crystalline mineral. Theoretically it should contain 87·1 per cent of silver and 12·9 per cent of sulphur; but in a specimen analysed by Klaproth, the proportions were 85 and 15.

Again, the analyses of pyrrhotine (magnetic pyrites) give results varying between 38·78 per cent sulphur, 60·52 per cent iron (variety from Bodenmais), and 43·63 sulphur, 56·37 iron (variety from Barèges). The constitution of the mineral is still uncertain; but its true formula is probably $Fe\,S$, which would require 36·4 per cent sulphur and 63·6 per cent iron. Lastly, the analyses of antimony-glance give results varying between

Antimony	74·06,	and	Antimony	73·5,
Sulphur	25·94,		Sulphur	26·5.

The true formula of this mineral is undoubtedly $Sb\,S^3$, which would require only 72·88 per cent of antimony.

Similar examples might be greatly multiplied. Those just cited were selected at random from the first few pages of Dana's 'System of Mineralogy.' They are all examples of binary compounds which occur almost chemically pure in nature; so that the phenomena in question are not complicated by those of isomorphism.

When we pass to minerals of more complex constitution, the same phenomena can be made evident, although not quite so easily, on account of the introduction of the phenomena of substitution by isomorphous elements. It will not, however, be necessary for me to cite examples; for it is a fact perfectly well known to all mineralogists, that, after making allowances for the substitution of isomorphous elements, the various analyses of such minerals as mica, hornblende, garnet, and tourmaline differ very greatly from each other,—a difference, moreover, which no mere error of analysis will explain, and which must therefore be referred to an actual variation in composition. In the silicates this variation in composition is made evident by the variation of what is termed the "oxygen ratios;" and it is well known to mineralogists that in many species this variation is very large.

For example, in mica the following ratios between the oxygen in the base and acid have been observed in merely the Muscovite variety:—13 : 16, $13\frac{1}{8}$: 16, and $14\frac{2}{8}$: 16; and similarly wide variations might be pointed out in other well known species. It is in consequence of such variations as these that the general chemical formulæ of some of the best known mineral species, such as mica and tourmaline, are still uncertain; and in other cases, where the true formulæ is probably known, the constitution of the mineral has been determined quite as much from other considerations as from the chemical analyses.

Sufficient has been said, I think, to show that variations in composition similar to those which I have observed in zinc and antimony occur in many minerals; and I trust that the results of my investigation will serve to throw light on this whole class of phenomena, which have so greatly perplexed mineralogists, and rendered all strictly chemical classifications of mineral species so unsatisfactory. This investigation has shown that a definite crystalline form is compatible with quite a wide variation of composition, and has in this way pointed out an explanation of the variation observed in the mineral kingdom. But more than this, the investigation has also indicated a method by which, amidst all this variation, the true constitution of the mineral can be determined.

In the compounds of zinc and antimony, although the definite crystalline form was compatible with a wide variation in the proportions of the constituent elements, yet the point corresponding to the typical composition was marked by several unmistakeable properties, which clearly enough indicated the true formulæ of the compounds. These properties are discussed at length in my original memoir, and need therefore only to be alluded to in this connection.

It has already been stated that the crystals, both of $Sb\,Zn^3$ and $Sb\,Zn^2$, having the theoretical composition are, as a rule, larger and more generally aggregated than those containing an excess of either metal. Moreover, in $Sb\,Zn^2$ the general character of the crystals appears to be modified by the change of composition, although the crystallographic elements remain the same. Thus in the crystals having the theoretical composition, the octahedral planes are greatly developed, giving to the crystals the general appearance of a truncated octahedron.* But as the crystals take up an excess either of antimony or zinc, the basal planes become more and more dominant, and the crystals are at last reduced to thin plates. In fact, so marked are these changes, that, after a little experience, a person could tell the approximate composition of the crystals from their general appear-

* See figure accompanying my original memoir.

ance. Similar changes in the appearance of many minerals are familiar to the mineralogist. They are seen in calcite, heavy spar, Anglesite, and others, and may serve as guides in tracing variations of composition.

Again, the specific gravity of the crystals, both of $Sb\,Zn^2$ and $Sb\,Zn^3$, was taken with great care through the whole series, and the results are tabulated below. The union of the two elements is attended with an increase of volume, and this increase is at a maximum at the points corresponding to the theoretical composition. These points would therefore be marked in a set of crystals by being points of minimum specific gravity; and they could be determined with great accuracy by means of this property, even in a series of alloys of the two metals which had not been crystallized. This fact is illustrated by the following Table, reprinted from the original memoir.

Specific Gravities of Crystals formed in the Alloys of Zinc and Antimony.

Composition of the alloys.		Composition of the crystals.		Spec. grav. of crystals by experiment.	Mean spec. grav. of zinc and antimony.	Expansion in crystallizing.
Per cent of Zn.	Per cent of Sb.	Per cent of Zn.	Per cent of Sb.			
10 00	.....	....	....	7·153	7·153	0·000
*96·00	4·00	....	....	7·069	7·133	0·064
*86 20	13·80	....	....	6 898	7·082	0·184
*76·30	23·70	....	....	6·769	7·032	0 263
70·40	29 60	64·20	35·80	6·699	6 975	0·276
66 50	33 50	61·00	39 00	6·628	6·959	0·331
64 50	35 50	58·56	41·44	6·596	6·948	0·352
62 50	37·50	55·53	44·47	6·506	6·933	0·427
60·60	39·40	55 00	45·00	6·440	6·931	0·491
58 60	41·40	50 39	49·61	6·396	6 909	0·513
56·60	43·40	49·95	50·05	6·388	6·906	0·518
48·70	51 30	48·66	51·34	6·404	6·900	0·496
46·70	53·30	46 77	53·23	6·376	6·891	0·515
44·80	55·20	44·26	55·74	6 341	6 879	0·538
†42·80	**57·20**	**43·09**	**56·91**	**6·327**	**6·874**	**0·547**
*40·00	60·00	....	....	6 386	6·860	0 474
*35·00	65·00	....	....	6 404	6·837	0·433
33 00	67·00	35·37	64·63	6·401	6·838	0·437
‡29·50	**70·50**	**33·62**	**66·38**	**6·384**	**6 830**	**0·446**
‡27·50	**72·50**	**33·85**	**66·15**	**6·383**	**6 831**	**0·448**
26·50	73 50	32 08	67·92	6·400	6 822	0·422
26·00	74 00	31·07	68·93	6·418	6·818	0·400
25·50	74·50	30·43	69 57	6 428	6 816	0·388
24·50	75·50	28·76	71·24	6·449	6 807	0·358
22·50	77·50	26·62	73·38	6·453	6 798	0 345
21·50	78·50	24·83	75·17	6·467	6·790	0·323
*15 00	85 00	....	....	5 564	6·744	0·180
*10 00	90·00	....	....	6·603	6·721	0·118
*5 00	95 00	....	....	6 655	6·698	0 043
....	100·00	....	....	6 677	6·677	0 000

* Alloys not crystallized. † Point of typical composition of $Sb\,Zn^2$.
‡ Point of typical composition of $Sb\,Zn^3$.

The point of typical composition in the case of the crystals of $Sb\,Zn^3$ was still further marked in a most decided manner by a very remarkable property. It has already been stated that this compound has the power of decomposing water with rapidity at 100° C.; but this is true only of those crystals which have approximately the theoretical composition. During the course of my investigation I determined the quantity of hydrogen evolved by alloys of different composition during a given time, taking care, of course, that the circumstances should be the same in all cases; and I found that with the alloy containing 43 per cent of zinc, there is an immense maximum, confined at most between 2 per cent on either side, the alloy of 43 per cent yielding over nine times as much gas as an alloy of 50 per cent, although the crystals of the last were fully as definite as those of the first.*

It is evident from the above facts, that the points corresponding to the theoretical composition of the two compounds of zinc and antimony, are also *points of maxima and minima* of various properties. Now I have no doubt that the same truth will be found to hold in the mineral kingdom. In a mineral like tourmaline or mica, for example, the specimen having the exact theoretical composition may probably be discovered by examining a large number of specimens, and discussing their various physical properties. All the physical properties may be of value in this connection, such as lustre, hardness, specific gravity, specific heat, &c.; and no mechanical rules can be laid down. Much must depend on the discretion of the observer; and in any cases such properties will be selected as are best adapted to the circumstances of the case. In comparing different crystals of the same mineral, it is obviously important to select such as have been formed in a different matrix; for it is only with such that we should be led to expect great variations of composition. It is also evident that the phenomena would be complicated when there has been a substitution of isomorphous elements; and until the effect of such substitution on the physical properties can be traced, it will be necessary to select specimens of as uniform a constitution as possible.

With one other consideration I will close this paper. The principle which has been here discussed must modify materially our notion of a mineral species. The idea of a mineral species has hitherto involved chiefly two distinct characters:—first, a definite crystalline form; second, a constant general formula; and any important variation in either of these characters has been regarded as equivalent to a change of species. Rutile and anatase are regarded as different species, because their crystalline forms are slightly different, although both minerals have identically the same constitution; and again, magnetite and Franklin-

* See Table in the memoir before cited.

ite, which have the same form, are regarded as different species, because they have a slightly different composition. It is true that the actual composition of a mineral may vary very greatly by the substitution of isomorphous elements, and yet, if the general formula remains constant, the species may not be changed. But the extent to which such substitution can be carried without changing the species is not so well settled among mineralogists as could be desired, and the same rule is not applied to all species. The difference between the *varieties* of garnet, for example, is as great as that between the *species* magnetite and Franklinite. Leaving, however, this point undetermined, all mineralogists have agreed that any *essential change* in the *general formula* was inconsistent with the idea of the *same species.* The result, however, of my investigation is to show that the general formula of a mineral species may vary also, or, as I should rather say, the *general formula* is not necessarily the actual formula of each given specimen, but only the *typical formula* of the species towards which the mineral tends, and which it would unquestionably reach if it could be several times recrystallized.

According to this view, the general formula represents not the *actual constitution* of the mineral, but only a certain *typical composition*, which perhaps is never realized with any actual specimen. The fact that the composition of a mineral species may be modified by the substitution of isomorphous elements, was first established by Mitscherlich, and has long been an admitted principle in mineralogy. We must now, as I think, still further expand our idea of a mineral species, and admit that its composition may be modified by an actual variation in the proportions of its constituents. Thus it is that in mineralogy, as in other sciences, we are led to admit the truth of that maxim which every advance in true knowledge seems to verify, "Natura non facit saltus."

While the results of my investigations thus serve to render the idea of a mineral species less definite than before, I cannot but hope that they will tend ultimately to simplify the whole subject of mineralogy; for not only may we expect to reduce the number of mineral species, but also, by simplifying the general formulæ of those which remain, to classify the whole with a greater precision than is now possible. To do this, however, implies a careful revision of the whole subject-matter of mineralogy on the principles above given,—a labor of which few can appreciate the extent, except those who are familiar with the methods of physical research. The work cannot be done by any one person; and it is the chief object of the present paper to call the attention of mineralogists to the importance of the subject.

I have not thought it necessary to dwell in this paper on the obvious distinction between the phenomena here in considera-

tion, and those of isomorphism. It was shown in my previous memoir, that the variation in the composition of the crystals of $Sb\,Zn^3$ and $Sb\,Zn^2$ could not be explained by this principle; and the distinction between the two classes of phenomena has been still further illustrated by a recent investigation on the crystals formed in alloys of copper and zinc, made in my laboratory by Mr. F. H. Storer. These crystals, which are undoubtedly mixtures of isomorphous elements, give no indications whatever of points of typical composition,—thus illustrating not only the characters of an isomorphous mixture, but also the distinction between such a mixture and a true chemical compound. Admitting, then, the possibility of a variation of composition in a mineral species, independent of the phenomena of isomorphism, it becomes of importance to distinguish this new class of phenomena by a separate term; and I would propose for this purpose the word *Allomerism.* By this word I would designate *a variation in the proportions of the constituents of a crystallized compound without any essential change in the crystalline form.* If, then, we also use the word *typical* to indicate the condition of *definite composition*, we may speak of those specimens of a mineral species which contain an excess of one or the other constituent, as *allomeric variations from the typical composition.* The degree of allomerism would then be measured by the excess of the *allomeric constituent* above the typical composition. Thus the crystals of $Sb\,Zn^3$ containing 42·3 per cent of zinc would be said to have the typical composition; while those containing 55 per cent of zinc would be distinguished as an allomeric variety, the degree of allomerism in this instance amounting to 12 per cent, and zinc being the allomeric constituent. In the case of the mineral Discrasite, it is probable that no specimen having the typical composition has yet been analyzed. Those specimens whose analyses are given in Dana's 'System of Mineralogy,' are all probably *allomeric varieties* of the mineral, silver being the allomeric constituent, and the degree of allomerism varying from 4 to 7 per cent. It is unnecessary, however, to multiply examples, as the above are sufficient to illustrate the use of the term.

ART. XIX.—*Notices of several American Meteorites;* by CHARLES UPHAM SHEPARD.

1. *Nebraska Iron.*—This very interesting mass, first noticed in a late number of the Proceedings of the St. Louis Academy of Sciences, was brought to St. Louis by the fur traders in the employ of C. P. Chouteau, Esq., about two years ago, and by him presented to the museum of the Academy. It was found near the Missouri River, between Council Bluff and Fort Union. It

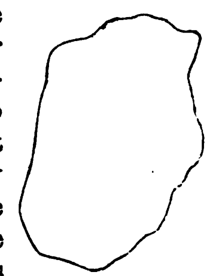
originally weighed about thirty-five pounds, but is now reduced to twenty-nine. Its shape was an oblong, compressed oval, not unlike that of the Chesterville, South Carolina, iron-mass, which has been compared by me to the form of a thick, blunt edged fresh water clam (*Unio*). Its surface is as black and smooth as that of the Braunau Iron, from which however it differs in being more even and smooth, though it is not destitute of the usual indentations belonging to meteorites, but these are by no means uniform in their occurrence over the entire surface. The crust is everywhere extremely thin, amounting to scarcely more than a mere varnish; and what is very remarkable, is often insufficient to hide the Widmannstättian figures with which the body must have been covered before it entered our atmosphere. The lines are not equally displayed throughout, and indeed will generally require a single lens in order to be distinctly seen. Nor have they the same beautiful regularity as when obtained by etching upon a polished surface from the interior. They are moreover curiously knotted, so as to resemble, under the microscope, the blunted teeth of a fine saw blade. The configuration upon the etched plates of this iron resembles slightly that of the Texas mass, though the bars are much more rectilinear, and in this respect approach nearer to the new African irons described by me (those from Namaqua Land and Orange River). I observe, however, in this as well as in most other irons, that the fullest regularity of internal structure does not prevail, until some little depth from the outer surface or crust is reached. The Nebraska iron is quite free from earthy, plumbaginous or pyritic matter. Prof. Litton, of the St. Louis University, has recognized in it the presence of nickel; and is understood to be now engaged with its complete analysis. Its specific gravity is 7·735. The character of the surface renders it certain that this mass must be of very recent fall. I am indebted to the liberality of the Academy, and the kind offices of Nathaniel Holmes, Esq., its Secretary, for a fine slice of the mass, from which I have been enabled to make the foregoing observations.

2. *Forsyth* (Taney County, Missouri) *Iron.*—My first information of this locality was derived, while on a visit to southeastern Missouri in April last, from N. Aubushon, Esq., of Ironton. He stated that a small specimen of curiously knitted, malleable ore, of a white color resembling silver, had been sent him two or three years ago by a person residing near the locality. Mr. Aubushon forwarded it to an assayer at Ducktown, Tenn., from whom he learned that it was composed of iron and nickel. On visiting St. Louis soon after, I was informed by Prof. Swallow, the State Geologist, that a specimen had also been transmitted to him by letter from the same place; and that Prof. Litton had found it to be composed of similar constituents. Prof. Swallow

presented me a small fragment of his specimen, upon which I am able to offer a few remarks, awaiting the results of Prof. Litton's analysis, for fuller information.

The mass evidently belongs to the rather rare group of amygdaloidal meteoric irons, in which, like those of Steinbach (Saxony) and Hainholz (Westphalia), the peridotic ingredient preponderates over the nickelic iron. Its specific gravity is 4·46. The iron is remarkable for its whiteness, while the peridot is of a well marked green color, and distinctly crystalline. No pyrites is visible in the very small fragments examined. It is reported that two considerable masses of this meteorite were found buried in the soil upon a hill-side; and that they are at present secreted under the belief that they contain silver.

3. *Bethlehem* (New York) *Meteoric Stone*, of Aug. 11, 1859.—The only stone found from the great explosion heard over a large district of northwestern Massachusetts, and extending into the state of New York as far as ten miles west of the cities of Albany and Troy, was the little fragment, less in size than a pigeon's egg, of which an outline is here subjoined.



I am imdebted to David A. Wells, Esq., the editor of the American Scientific Annual, for several interesting particulars concerning its discovery and properties. He was good enough to visit, at my request, the residence of Mr. Garritt Vanderpool (situated seven miles from Albany and one mile west of Bethehem church), where the stone fell, and to ascertain on the spot the facts respecting its descent. Mr. Vanderpool was at work near his house, and heard the explosion in common with other members of his family. About two minutes after, as it appeared to him, a stone, coming in an oblique course, struck the side of a waggon-house, glanced off, hit a log upon the ground, bounded again, and rolled into the grass. A dog lying in the doorway of the waggon-house sprang up, darted out and seized it, but dropped it immediately, probably on account of its warmth and sulphureous smell. Mr. Wells had two opportunities of inspecting the stone before it was sold to the State Cabinet in Albany. It was far from being entire when first picked up, no doubt having been broken by its contact with the house. On the second inspection, he noticed that one corner had been broken away, and other portions much marred through the use of knife blades upon its surface by the curious, who, in this rude way, had been led to investigate its peculiarities. About "one-half of it however," he observes, "is covered with the peculiar dark colored crust of meteorites, and has a burnt appearance. This is so well marked that it at once establishes its identity as a meteoric stone. The other sides present-

ing the appearance originally bright and of a fresh fracture were clear, but are now soiled from handling. The color is a light steel-grey, with metallic particles interspersed. The structure is granular."

Through the recommendation of His Excellency, Gov. Morgan, to the officers having in charge the state cabinet, a small fragment of the stone including a portion of the crust, was most obligingly transmitted to me by Mr. Woolworth, accompanied by the following note:

"Albany, Nov. 11, 1859.

Prof. Charles U. Shepard,

Dear Sir:—I am directed by Gov. Morgan, as Chairman of the Committee of the Regents of the University on the State Cabinet of Natural History, to send you the inclosed portion of the aërolite lately found near this city. The Committee had hopes of finding other parts of the stone than the one first discovered, but have not been successful. They regret they cannot send you more, but could not do so without destroying the specimen they possess. Hoping it may be sufficient for your purposes, I am, very truly, yours, &c.,

J. B. Woolworth, *Sec'ry, &c.*

I am likewise much indebted to Henry A. Homes, Esq, the State Librarian, for his good offices in facilitating my early acquisition of the specimen which enables me to compare it with those I possess from other localities.

The crust of the Bethlehem stone is very peculiar. It is double the thickness of any in my collection, equalling that of thick pasteboard. It is perfectly black, and very open in its texture. The outer surface is rough, being nowhere perfectly fused, but only semi-vitrified. Without being fragile or carbonaceous, it nevertheless resembles in color, lustre, and porousness, certain surfaces of mineral charcoal. The interior of the stone is equally peculiar, being loosely granular, the particles being uniform in character, small, highly crystalline, and nearly transparent. They possess a brilliant lustre, a very light grey or greenish white color. They resemble volcanic peridot more than any species of the augitic or feldspar family. Nickelic iron, of a bright white color, in delicate filaments and semi-crystalline grains, is thickly diffused through the mass; and these grains, as well as those of the peridotic mineral, are flecked with brilliant points of pyrrhotine (FeS). The specific gravity is 3·56. In general color and effect to the eye, it approaches nearest to the Klein-Wenden stone (Sept. 16, 1843); but it differs from this in being larger grained, and looser in its texture.

4. *Remarks upon the Ohio stones of May* 1, 1860.—Through the much valued assistance of Prof. J. L. Smith, the large 53-pound stone that fell near the house of Mr. Wm. Law of New Concord, forms part of my meteoric cabinet. Without attempting at present a complete description of its form and character,

I will only offer a few remarks upon the relationship of the Ohio meteorites to those of other falls. In its internal aspect it approaches the stone of Jekaterinoslaw, Russia (1825), though it is somewhat firmer and more compact. In crust, the two are identical. It is also similar to the stone of Slobodka, Russia (Aug. 10, 1808); and compares closely with those of Politz (Oct. 13, 1819), of Nanjemoy, Maryland (Feb. 10, 1828), and of Kuleschowka, Russia (March 12, 1811); but the crust is less smooth on the Ohio stone than in that of the latter.

A pearl grey peridot forms the chief constituent (above two-thirds) of the stone. This mineral is often rolled up into obscurely formed globules, which are so firmly imbedded in the more massive portions of the same mineral, as to be broken across on the fracture of the stone, which thereby presents a sub-pisiform appearance. Snow white particles of Chladnite are thickly scattered in mere specks through the mass, and closely incorporated with the peridot. The nickelic iron, of a bright white color, is also everywhere thickly interspersed in little points. Pyrrhotine is less conspicuous, though often visible in rather broad patches; while black grains of chromite are easily distinguishable by the aid of a glass, and sometimes with the naked eye.

The crust is of medium thickness, and the usual wavy and pitted impressions are also strictly characteristic of these stones. Their origin in meteorites generally, is perhaps still obscure, but may be conceived to originate in the flaking off of fragments in consequence of the sudden transition from cold to hot, which must happen to bodies coming instantaneously from a temperature far below zero into a state of vivid incandescence, at least upon their immediate surface. We see a somewhat analogous flaking up from heated surfaces of granite blocks during a conflagration, when wetted by cold water; though in the latter case, as might be expected, convexities take the place of concavities.

5. *Supposed Fall of a Meteoric Stone in Independence County, Iowa, during the summer of* 1857.—I casually learned while recently in Missouri, that a stone fell at a place called Pilot Grove, near the stage road, in or near the month of August, 1857. The stone was preserved; and I am not without hopes of obtaining a portion of it, having heard of its exhibition during the last year before the Academy of Sciences at Chicago.*

* *Detection of Phosphorus in the native steel of Montgomery (Vermont), and in the Waterloo (New York) Meteoric stone:*

I have examined the first named substance chiefly with a view to determine its relationship to the Rutherfordton (N. C.) Ferrosilicine, (see the September number of this Journal for 1859), and find that while it is free from silicon, it nevertheless abounds in phosphorus. The Waterloo stone, whose resemblance is so great to a well burnt Bristol brick, gives a very decided test for phosphoric acid. The problematical steel from Bedford County (Pennsylvania) is free both from silicon and phosphorus.

New Haven, July 1, 1860.

Art. XX.—*Influence of Arsenious Acid upon the Waste of the Animal Tissue.*

According to experiments made by Prof. Schmidt and Dr. Stuerzwage of Dorpat,* arsenious acid when introduced into the circulation, occasions a considerable diminution of the ordinary waste of the tissues.

This decrease, which amounts to from twenty to forty per cent, occurs even after the administration of very small doses; more rapidly if the acid is injected directly into the veins; more slowly, yet with equal intensity, if absorbed from the intestines. The action is most striking in the case of fowls which neither vomit after injection of the arsenic nor reject their accustomed food; but even in cats which are subject to vomiting after the injection and must therefore be regarded as in a starving condition, the waste of the organism was diminished about twenty per cent after subtracting the decrease occasioned by the mere want of food.

This fact satisfactorily explains the fattening of horses after small doses of arsenious acid, a phenomenon well known to horse dealers.

An amount of fat and albuminous substances equivalent to the repressed carbonic acid and urea remains in the body and increases its weight, if the animal receives at the same time a sufficient amount of food.

When larger doses of arsenious acid are given nervous symptoms appear, which may be classified in two groups: spinal irritation and paralysis. To the first may be referred the vomiting, the accelerated respiration, the feeble pulse; to the last, the inclination to sleep, the weakness, and the retarded and labored breathing. Both may be explained by the very considerable congestion of the central organs which was constantly observed in post mortem examinations.

These experiments are of particular interest since they go far to prove the complete reliability of the published accounts of the custom of "arsenic eating," which is said to prevail among the peasantry of several Austrian provinces. These accounts have been time and again held up to ridicule by toxicologists,† and as a rule have been received with suspicion by all scientific men. They have nevertheless been widely published and are consequently well known to the public.

During the last eight or ten years the origin of these accounts

* *Journal für praktische Chemie*, 1859, lxxviii, p. 373.

† See for example, *Christison*, *Edinburgh Medical Journal*, Feb. 1856, i, 709. *A. Chevallier*, *Journal de Chimie Médicale, etc.*, 1854, [3.] x, 439. Or *Taylor*, in his work *On Poisons*. London, Churchill, 1859, p. 91.

has been generally attributed to Dr. v. Tschudi, who published a communication upon the subject in 1851;* an abstract of which may be found in Chambers' Edinburgh Journal, December 20, 1851, [N. S., No. 416,] p. 389.†

Two years later, v. Tschudi made another communication,‡ in support of his previous assertions:—this, in connection with his first letter which had previously attracted comparatively little attention, was very extensively copied.§

Similar stories had been circulated, however, long before the letters of v. Tschudi were made public. For example, our own attention was first directed to the subject by the statement published in the Penny Cyclopædia of the Society for the Diffusion of Useful Knowledge, London, 1832, ii, 403. Art. "*Arsenic, Medical Uses of.*" * * *

"That its [white oxyd of arsenic] employment in such doses [$\frac{1}{30}$ or $\frac{1}{15}$ of a grain] as we have stated is not only safe but beneficial, may be satisfactorily proved. Not only are old worn out horses endowed with new vigor, improved appetite, &c., by its use, but pigeons to which this article is given, show greater appetite and liveliness than others without it; and in Upper Styria the peasantry use it as a seasoning with many articles of food, such as cheese." * * *

In the Boston Medical and Surgical Journal, 1835, xii, 211, is the following:

"Dr. Strohmayer in his Medicinische Praktische,‖ relates in exemplification of the extent to which the system may become accustomed to the operation of arsenic, that a peasant who resided near a convent in the Tyrol, for a long time, took ten grains of arsenic daily with his food." * *

In noticing the article in Chambers' Journal, for 1851, a correspondent of the London and Edinburgh Monthly Journal of Medical Science, February, 1852, xiv, 190, cites the following extracts:

* *Wiener medizinische Wochenschrift*, October 11th, 1851, vol. i, No. 28.

† From which it was copied into Wells's Annual of Scientific Discovery, 1852, p. 362.—Hays's American Journal of the Medical Sciences for July, 1852, vol. xxiv, p. 270, also contains extracts of v. Tschudi's letter, taken from the French *Gazette des Tribunaux* through the *Journal des connaissances Med. Chirurg.*, December 16th, 1851.

‡ *Wiener medizinische Wochenschrift*, 1853, No. 1.

§ *In extenso* in *Journal de Chimie Médicale, etc.*, 1854, [3.] x, 439; from *La Presse Médicale Belge;* from *Journal de la Société des Sciences médicales et naturelles de Bruxelles;* abstract in Chambers's *Edinburgh Journal*, June 11th, 1853, [N. S.,] vol. xix, No. 493, p. 382.

An abstract of the first (1851) letter, in the *Gazette de Hôpitaux*, of Paris, May 16, 1854, p. 229; from *Journal de Médicine de Bruxelles*, (see also *London Medical Times and Gazette*, July 1854, xxx, 66,) is perhaps the best known of any of the numerous extracts from v. Tschudi's statements, unless it be that given by J. F. W. Johnston in his Chemistry of Common Life, New York, Appleton, 1855; i, 166; also in Blackwood's Magazine, Dec. 1853, lxxiv, 687.

[‖ Quere? Strohmayr, Frz. *Medicinische praktische Dartstellung gesammelter Krankheitsfälle, u. des Heilverfahrens aus dem Tagebuch meiner Erfahrung Wien, Gerold*, 1831.]

"From Vogt's,* Arzneimittellehre, B. 1, S. 507." "It is well-known that old worn out horses gain an appetite, strength and spirit by the use of arsenic; and a pigeon which often got arsenic was observed to have its appetite increased and its movements more lively."

"From *Med. Jahb. des Oester. Staates*, 1822, S. 99." "There is scarce a district of Upper Styria in which in at least one house, arsenic may not be found under the name of Hydrach, Orpiment, &c. It is used for diseases of the domestic animals, against vermin, and also as a stomachic to increase the appetite. A peasant in my presence showed, with the point of a knife, how much arsenic he took daily and without which he said he could not live. I estimated the quantity at about two grains. It is also said to be used as a seasoning for cheese, and indeed several cases of poisoning by Styrian cheese have occurred, and one but lately."

Similar statements made by Wibmer, (probably in his book, entitled, "*Die Wirkung der Arzneimittel, u. Gifte im gesunden thierischen Körper*," 4 vols., Munich, 1831–39,) are referred to in German works upon the materia medica; while travellers who have spent much time in these provinces, all concur in their statements regarding the common custom of mixing arsenic with the food of horses.†

Evidence of this sort could without doubt be multiplied to almost any extent by any one familiar with the literature of the provinces in question, or with the habits of their people. A quantity of such material,‡ has indeed been recently collected by Heisch, and published in the London Chemical News, May 19, 1860, i, 280; from which we quote it, as being recent, (for the most part,) precise and tolerably direct; although it does not differ in its general import from the testimony which had already been offered.

On the Arsenic Eaters of Styria, by Charles Heisch, Esq., F.C.S., Lecturer on Chemistry at the Middlesex Hospital Medical College.§—At the last meeting of the Manchester Philosophical Society I observe that Dr. Roscoe called attention to the arsenic eaters of Styria. Having for the last two years been in communication with the medical men and other residents in the districts where this practice prevails, I shall feel obliged if you will allow me through your journal to make known the facts I have at present collected. The information is derived mainly from Dr. Lorenz, Imperial Professor of Natural History, formerly of Salzburg, from Dr. Carl Arbele, Professor of Anatomy in Salzburg, and Dr. Kottowitz, of Neuhaus, besides several non-medical friends. If human testimony be worth anything, the fact of the existence of arsenic eaters is placed beyond a doubt. Dr. Lorenz, to whom questions were first addressed, at once stated that he was aware of the practice, but added, that it is generally difficult to get hold of individual cases, as the obtaining of arsenic

* [Qu.? Voigtel, Fr. G. *System der Arzneimittellehre*, Leipzig, 1816.]

† A custom which seems also to prevail to a certain extent in England. Compare Kesteven, cited by Taylor, (*op. cit.*, p. 89,) from the Association Medical Journal, Sept. 6, and 20, 1856. We have to regret our inability to refer to K.'s original paper, the tenor of which is not readily to be inferred from Dr. Taylor's extracts.

‡ Compare Boner of Ratisbon in Chambers's Journal of Pop. Lit., &c., Feb. 9th, 1856, vol. v, No. 110, p. 90; see also *ibid*, July 19th, 1856, vol. vi, No. 133, p. 46.

§ From the Chemical News, May 19th, 1860.

without a doctor's certificate is contrary to law, and those who do so are very anxious to conceal the fact, particularly from medical men and priests. Dr. Lorenz was, however, well acquainted with one gentleman, an arsenic eater, with whom he kindly put me in communication, and to whom I shall refer again more particularly. He also says that he knows arsenic is commonly taken by the peasants in Styria, the Tyrol, and the Salzkammergut, principally by huntsmen and woodcutters, to improve their wind and prevent fatigue. He gives the following particulars:—

"The arsenic is taken pure when fasting, in some warm liquid, as coffee, beginning with a bit the size of a pin's head, and increasing to that of a pea. The complexion and general appearance are much improved, and the parties using it seldom look so old as they really are, but he has never heard of any case in which it was used to improve personal beauty, though he cannot say that it never is so used. The first dose is always followed by slight symptoms of poisoning, such as burning pain in the stomach and sickness, but not very severe.

"Once begun, it can only be left off by very gradually diminishing the daily dose, as a sudden cessation causes sickness, burning pains in the stomach, and other symptoms of poisoning, very speedily followed by death.

"As a rule, arsenic eaters are very long lived, and are peculiarly exempt from infectious diseases, fevers, &c., but unless they gradually give up the practice invariably die suddenly at last.

"In some arsenic works near Salzburg with which he is acquainted, he says the only men who can stand the work for any time are those who swallow daily doses of arsenic, the fumes, &c., soon killing the others. The director of these works, the gentleman before alluded to, sent me the following particulars of his own case. (This gentleman's name I suppress, as he writes that he does not wish the only thing known about him in England to be the fact that he is an arsenic eater; but if any judicial inquiry should arise which might render positive evidence of arsenic eating necessary, his name and testimony will be forthcoming):—

"'At seventeen years of age, while studying assaying, I had much to do with arsenic, and was advised by my teacher, M. Bönsch, Professor of Chemistry and Mineralogy at Eisleben, to begin the habit of arsenic eating. I quote the precise words he addressed to me:—'If you wish to continue the study of assaying, and become hereafter superintendent of a factory, more especially of an arsenic factory, in which position there are so few, and which is abandoned by so many, and to preserve yourself from the fumes, which injure the lungs of most, if not of all, and to continue to enjoy your customary health and spirits, and to attain a tolerably advanced age, I advise you, nay, it is absolutely necessary, that besides strictly abstaining from spiritous liquors, you should learn to take arsenic; but do not forget, when you have attained the age of fifty years, gradually to decrease your dose, till from the dose to which you have become accustomed you return to that with which you began, or even less.' I have made trial of my preceptor's prescriptions till now, the forty-fifth year of my age. The dose with which I began, and that which I take at present, I enclose; they are taken once a day, early, in any warm liquid, such as coffee, but not in any spirituous liquors.' The doses sent were No. 1, original dose, three grains: No. 2, present dose, twenty-three grains of pure white arsenic in coarse powder. Dr. Arbele says this gentleman's daily dose has been weighed there also, and found as above. Mr. —— continues:—'About an hour after taking my first dose (I took the same quantity daily for three months,) there followed slight perspiration with griping pains in the bowels, and after three or four hours a loose evacuation; this was followed by a keen appetite and a feeling of excitement. With the exception of the pain, the same symptoms follow every increase of the dose. I subjoin, as a caution, that it is not advisable to begin arsenic eating before the age of twelve or after thirty years.' In reply to my question, if any harm results from either interrupting, or altogether discontinuing the practice, he replies, 'Evil consequences only ensue from a long continued interruption. From circumstances I am often obliged to leave it off for two or three days, and I feel only slight languor and loss of appetite, and I resume taking the arsenic in somewhat smaller doses. On two occasions, at the earnest solicitations of my friends, I attempted entirely to leave off the arsenic. The second time was in January 1855. I was induced to try it a second time from a belief that my first illness might have arisen from some other cause. On the third day of the second week after leaving off the dose I was attacked with faintness, depression of spir-

its, mental weakness, and a total loss of the little appetite I still had; sleep also entirely deserted me. On the fourth day I had violent palpitation of the heart, accompanied by profuse perspiration. Inflammation of the lungs followed, and I was laid up for nine weeks, the same as on the first occasion of leaving off the arsenic. Had I not been bled, I should most likely have died of apoplexy. As a restorative, I resumed the arsenic eating in smaller doses, and with a firm determination never again to be seduced into leaving it off, except as originally directed by my preceptor. The results on both occasions were precisely the same, and death would certainly have ensued had I not resumed arsenic eating.' One of the most remarkable points in this narrative is that this gentleman *began* with a dose which we should consider poisonous. This is the only case of which I have been able to obtain such full particulars, but several others have been mentioned to me by those who knew the parties, and can vouch for their truth, which I will briefly relate.

"One gentleman, besides stating that he is well aware of the existence of the practice, says he is well acquainted with a brewer in Klagenfürth who has taken daily doses of arsenic for many years; he is now past middle life, but astonishes every one by his fresh juvenile appearance; he is always exhorting other people to follow his example, and says, 'See how strong and fresh I am, and what an advantage I have over you all! In times of epidemic fever or cholera, what a fright you are in, while I feel sure of never taking infection.'

"Dr. Arbele writes, 'Mr. Curator Kürsinger (I presume curator of some museum at Salzburg,) notwithstanding his long professional work in Lungau and Binzgau, knew only two arsenic eaters, one the gentleman whose case has just been related, the other the ranger of the hunting district in Grossarl, named Trauner. This man was at the advanced age of 81 still a keen chamois hunter and an active climber of mountains; he met his death by a fall from a mountain height while engaged in his occupation. Mr. Kürsinger says he always seemed very healthy, and every evening regularly, after remaining a little too long over his glass, he took a dose of arsenic, which enabled him to get up the next morning perfectly sober and quite bright. Professor Fenzl of Vienna was acquainted with this man, and made a statement before some learned society concerning him, a notice of which Mr. Kürsinger saw in the *Wiener Zeitung*, but I have not been able to find the statement itself. Mr. Krum, the pharmaceutist here, tells me that there is in Stürzburg a well known arsenic eater, Mr. Schmid, who now takes daily twelve, and sometimes fifteen, grains of arsenic. He began taking arsenic from curiosity, and appears very healthy, but always becomes sickly and falls away if he attempts to leave it off. The director of the arsenic factory before alluded to is also said to be very healthy, and not to look so old as forty-five, which he really is.'*

"As a proof how much secrecy is observed by those who practise arsenic eating, I may mention that Dr. Arbele says he inquired of four medical men, well acquainted with the people of the districts in question, both in the towns and country, and they could not tell him of any individual case, but knew of the custom only by report.

"Two criminal cases have been mentioned to me, in which the known habit of arsenic eating was successfully pleaded in favor of the accused. The first, by Dr. Kottowitz of Neuhaus, was that of a girl taken up in that neighborhood on strong suspicion of having poisoned one or more people with arsenic, and though circumstances were strongly against her, yet the systematic arsenic eating in the district was pleaded so successfully in her favor, that she was acquitted, and still lives near Neuhaus, but is believed by every one to be guilty. The other case was mentioned by Dr. Lorenz. A woman was accused of poisoning her husband, but brought such clear proof that he was an arsenic eater, as fully to account for arsenic being found in the body. She was, of course, acquitted.

"One fact mentioned to me by some friends is well worthy of note. They say: 'In this part of the world, when a graveyard is full, it is shut up for about twelve years, when all the graves which are not private property by purchase are dug up, the bones collected in the charnel-house, the ground ploughed over, and burying begins again. On these occasions the bodies of arsenic eaters are found almost un-

* The man above mentioned seems quite to differ with Mr. —— on the impropriety of taking arsenic with spiritous liquors, and actually employs it as a means of correcting their effects. All others that I have heard of concur in saying that it should be taken fasting.

changed, and recognisable by their friends. Many people suppose that the finding of their bodies is the origin of the story of the vampire.' In the *Medicinischer Jahrbuch des Oster. Kaiserstaates*, 1822, *neuest Folge*, there is a report by Professor Schallgruber, of the Imperial Lyceum at Grätz, of an investigation undertaken by order of government into various cases of poisoning by arsenic. After giving details of six post-mortem examinations, he says:—'The reason of the frequency of these sad cases appears to me to be the familiarity with arsenic which exists in our country, particularly the higher parts. There is hardly a district in Upper Styria where you will not find arsenic in at least one house under the name of hydrach. They use it for the complaints of domestic animals, to kill vermin, and as a stomachic to excite an appetite. I saw one peasant show another, on the point of a knife, how much arsenic he took daily, without which, he said, he could not live; the quantity I should estimate at two grains. It is said, but this I will not answer for, that in that part of the country this poison is used in making cheese; and, in fact, several cases of poisoning by cheese have occurred in Upper Styria, one not long since. The above-mentioned peasant states, I believe truly, that they buy the arsenic from the Tyrolese, who bring into the country, spirits and other medicines, and so are the cause of much mischief.' This report is, I believe, mentioned in Orfila's *Toxicology*, and one or two other works, but I have not seen it quoted myself; it is interesting, as being early and official evidence of arsenic eating. Since I received the above information, a gentleman who was studying at this hospital, told me that, when an assistant in Lincolnshire, he knew a man who began taking arsenic for some skin disease, and gradually increased the dose to five grains daily. He said he himself supplied him with this dose daily for a long time. He wrote to the medical man with whom he was assistant, and I have been for a long time promised full particulars of the case, but beyond the fact that he took five grains of arsenic, in the form of Fowler's solution, daily, for about six years, and could never leave it off without inconvenience and a return of his old complaint, I have as yet not received them. I have delayed publishing these facts for some time, hoping to get information on some other points, for which I have written to my friends abroad; but as considerable delay takes place in all communications with them, I have thought it better to publish at once the information I have already received. All the parties spoken of are people on whom the fullest reliance can be placed, and who have taken much pains to ascertain the foregoing particulars. The questions which still remain unanswered are these:—

"1st. Can any official report be obtained of the trials of the two people mentioned by Drs. Kottowitz and Lorenz?

"2nd. Do medical men in these districts, when using arsenic medicinally, find the same cumulative effects as we experience here? Or is there anything in the air or mode of living which prevents it?

"3rd. Can any evidence be obtained as to how much of the arsenic taken is excreted? to show whether the body gradually becomes capable of enduring its presence, or whether it acquires the power of throwing it off?*

"I have proposed to the gentleman who furnished me with the particulars of his own case either to make an estimate of the arsenic contained in his own urine and fæces during twenty-four hours, or to collect the same and forward them to me that I may do so, but as yet have received no answer."—*Pharmaceutical Journal.*

The only wonder is that direct experiments have not been made long ago upon the excretions of suspected arsenic eaters, or upon their bodies after death. In calling attention to the subject some months since, (*Répertoire de Chimie Appliquée*, February, 1860, ii, 44,) we took occasion to urge this point and to bring forward the observation of Prof. E. Kopp†, who found in the course of his experiments upon arsenic acid—which was manufactured upon the great scale and largely employed in calico-print-

* The fact of the preservation of the bodies shows that some considerable quantity must be retained [or rather offers an example of the well known fact that corpses loaded with fat decompose but slowly.—F. H. S.]

† Comptes Rendus, 1856, xlii, 1063; *J. pr. Ch.* lxix, 273.

ing by him—that the weight of his body rapidly increased, some 20 lbs. having been gained in the course of the two months, during which he was subject to absorb the acid, his hands having been frequently in contact with the arsenical solution: arsenic being detected the while in his solid and liquid excrements. As soon, however, as the exposure to the arsenic ceased, his weight began to decrease, and in the course of 9 or 10 weeks, fell back again to its normal—150 lbs. Believing that direct, positive evidence like this—though the instance be solitary—where the subject of the experiment was a healthy, vigorous man, and a trained observer, ought to outweigh almost any amount of negative testimony, such as has been brought forward by physicians who have not witnessed similar effects upon their *diseased* patients when the latter were treated with arsenical preparations. It should be observed that one of the strongest arguments brought up by toxicologists against the truth of the accounts of the arsenic eaters has been drawn from the result of medical practice. Without attempting to discuss the matter at length, it may, nevertheless be permitted to the non-professional reader of the medical literature bearing upon this subject to remark that the evidence there accumulated in spite of its apparent obscurity and of the contradictions with which it is involved seems to point clearly to the fact that in minute doses arsenious acid acts beneficially upon the general health of many patients. Thus, according to Dr. Henry Hunt:*

"Arsenic operates most favorably on persons who are of lax fibre, accompanied by a languid state of the circulation and whose secretions are rather profuse than otherwise; the urine pale and plentiful, and more especially on those whose skin is cold and moist. In persons of this description, whilst arsenic to an extent far beyond other medicines, relieves the neuralgic pain, it improves the general health, and gives firmness and vigor to the constitution." * * * * While upon certain peculiar temperaments it is hurtful; thus Dr. Hunt continues, "when neuralgia is associated with the same morbid action in the spine, with anæmia; or arises from injuries of nerves; or local irritation of nerves by diseases, or unnatural growth of bone; or if it be complicated with engorgement of the liver and other viscera; arsenic is usually injurious, and I believe seldom useful."

Again, not to multiply instances further, Erichsen,† in treating of the use of arsenic in diseases of the skin, "lays great stress upon the necessity of attending particularly to the constitution, and temperament of the patient before commencing the use of the medicine. It will be badly borne by individuals of a plethoric habit of body or of a highly sanguine or sanguineo-nervous temperament,—this arises from the stimulating properties of the metal. In such cases, the digestive organs become so ir-

* In his work upon Neuralgic Disorders, cited in Braithwaite's Retrospect of Pract. Med., 1844, No. ix, p. 34; also, *ibid.* No. x, p. 23.

† Braithwaite's Retrospect, 1843, No. viii, p. 14.

ritated and the nervous system so excited under the use of the arsenic that it is impossible to employ it in any such dose as can be expected to produce a beneficial effect upon the cutaneous affection. There are other circumstances which contra-indicate the use of this remedy, namely, 'the complication of the cutaneous affection with other diseases,' and especially with irritative or inflammatory gastric dyspepsia, accompanied with a sensation of heat and oppression at the epigastrium, increased by food so well described by Dr. Todd. When this form of dyspepsia is present the smallest doses of arsenic will do harm, as the usual effect of the remedy when continued too long is to produce these very symptoms. Besides this form of indigestion any other local inflammatory condition of the system or the superinvention of phthisis, will contra-indicate the use of so powerfully stimulating a tonic as arsenic." Further on he remarks: "It has already been shown that the use of the preparations of this metal is exceedingly hazardous in individuals of a sanguine or sanguineo-nervous temperament and excitable habit of body, or in those who suffer from or are peculiarly disposed to irritative gastric dyspepsia, or any inflammatory disorder. On the other hand, they are in most cases borne well by individuals of a somewhat phlegmatic, debilitated, or lax habit of body, more particularly, if they are past the middle age, with a pale cachectic complexion, languid weak circulation, and a general want of tone about the system, acting upon such patients as powerful and useful tonics. In persons of this habit of body the diseases of the skin appear rather to be dependent upon a degree of debility or want of power in the cutaneous capillaries; and it is in these patients that the preparations of arsenic are of great service in exciting in a peculiar manner a more healthy action in this class of vessels, thereby modifying or removing those morbid changes that are the results of an abnormal condition in their secernent and nutrient functions."

In this connection it must not be forgotten that in the opinion of many scientific men, the healing action of various mineral waters may depend, in part, at least, upon the arsenic which these springs are known to contain.* A doctrine which is publicly taught by several of the chemical Professors at Paris.

Taken as a whole the medical evidence which has fallen under our notice, is adverse to the possibility of "arsenic eating," only in so far as relates to the large quantities of the poison which, as is affirmed, the human body can accustom itself by long continued habit, to support with impunity. This last enquiry, however interesting in itself, is one on which very little is known with certainty as yet, and is plainly of quite secondary importance in a scientific point of view to that of the beneficial action of moderate doses of arsenious acid, which would now appear to be proved. From the very general interest which attaches to the subject it is greatly to be hoped that further researches may soon decide the amount of this tolerance. F. H. S.

* Compare *Walchner, Ann. Ch. u. Pharm.*, lxi, 206; or *Comptes Rendus*, xxiii, 614. *Figuier, Comptes Rendus*, xxiii, 820. *Chevallier* and *Schanefele, ibid.* xxv, 750. *Thenard, ibid.* xxxix, 769.

Art. XXI.—*Geographical Notices.* No. XIII.

Journal of the American Geographical Society.—The first number of the second volume of the American Geographical Society's Journal has appeared in a new and enlarged form, an octavo volume of 148 pages. The articles (eight in number) are of a more extended and scientific character than usual, and the whole appearance of the Journal is such as will reflect great credit on the society and enlarge its sphere of usefulness.

Article first, which is compiled from data furnished by the Hydrographical Office, Washington, gives an account of the progress of Marine Geography, within the past few years; which is followed by a synopsis of the operations of the Coast Survey during the year 1859, by Prof. Bache. An account of the lake Yojoa in Honduras, contributed by Mr. E. G. Squier, is the next article. The address commemorative of Karl Ritter, delivered before the Society by Prof. Guyot, comes next in order, forming the most complete and eloquent tribute to the great geographer which has yet appeared. Its review of the characteristics and contents of Ritter's Erdkunde is especially valuable. The sixth article is a translation by Mr. E. R. Straznicky from the Journal of the Geographical Society of France, of an essay on the Geographical distribution of Animals, by Mons. A. Maury. Mr. J. G. C. Kennedy, the superintendent of the U. S. Census, then reviews the origin and progress of Statistics, and Dr. Wynne illustrates the working of benevolent societies, such as the Odd Fellows, among the laboring classes. The number is concluded by an excellent and full survey of recent geographical and statistical literature, prepared by the General Secretary of the Society, Mr. D. W. Fiske, to whom, with the coöperation of the Committee of Publication, the editing of the Journal was entrusted. The Society now has a small but well selected library, with rooms in a central part of New York City. Its list of active members enrolls about five hundred names, and its usefulness and importance have never been greater than at present.

Schlagintweit's Mission to Central and High Asia.—We have received through Mr. S. H. Grant of New York, the prospectus of Mr. Brockhaus of Leipsic, announcing the contents and character of the Report which is soon to be published by the brothers Schlagintweit on their journey to the Himalayas, from 1854 to 1858. It contains some information to which we have not had access in our previous notices of their expedition. Since the return of the authors from India in June 1857, they have been engaged in preparing for publication the results

of their observations, and are now able to promise a work in nine volumes, quarto, with an atlas in three folio volumes. The importance of this work will justify us in explaining its character at some length.

The first volume will be devoted to Astronomy and Magnetism. The observations extend from Ceylon to Turkistan and from Assam to Kabul. Their importance may be the better appreciated if it is borne in mind that "with the exception of the well known observations at the government observatories of Bombay and Madras, some very valuable ones by Taylor and Caldecott in Southern India, and others recently made by Brown at Travankor, scarcely any observations have been taken in the interior of India, so that the modifications of the magnetic lines over this large area form a new object of scientific discussion."

In India proper the astronomical labors of the Schlagintweits relate chiefly to the determination of the true and magnetic meridian and to observations for finding time. But in the Himalaya, their operations included also determinations of latitude and longitude. The whole of Western Thibet has been found to be farther west than has hitherto been supposed, and for Kuenluen and Turkistan the latitudes have also been largely corrected.

The second volume will contain the hypsometrical and trigonometrical observations, including the determination of about 2000 points in the various countries explored. The third volume, on Topical Geography, has for its object a practical aim, reviewing chiefly the commercial and military routes in High Asia, with reference to their commercial and military importance. Part of this volume is devoted to linguistic researches and vocabularies. The fourth and fifth volumes include all that the explorers have collected on Meteorology, and the sixth is devoted to Geology. Volume seventh relates to Botany and Zoology. Volume eighth is given to Ethnography, including an examination and comparison of the facial casts to which we have previously referred in this Journal. The ninth and last volume presents in a popular form comparative descriptions of the various regions of India and High Asia.

The Atlas will contain: 1. Maps, geographical, physical, and geological; 2. Profiles, meteorological, hydrographical and geological; and 3. Views and general panoramas. The general size of the plates is three feet by two. The whole cost of the work will be £36, and its completion is promised in about three years.

CANADIAN EXPEDITION TO THE RED RIVER UNDER GLADMAN, DAWSON, HIND, AND NAPIER, 1857–1858.—The interest which has been manifested in the report of the Palisser expedi-

tion contained in a recent number of this Journal,* leads us to condense and translate from Dr. Petermann's excellent *Mittheilungen* (January, 1860) an account of the explorations of the Red River which were made in 1857 and 1858 by Gladman, Dawson, Hind, and Napier. We regret that we cannot reproduce the admirable maps which accompany the article. The writer in Petermann's Journal remarks substantially as follows:

Although the Canadians had long endeavored to direct the attention of the British government to that vast portion of British North America, which stood until very recently under the immediate supervision of the Hudson's Bay Company, and had tried to induce them to effect a revision of the claims of that mercantile body, it was nevertheless, not until 1856 when gold was discovered in Fraser's and Thompson's rivers, that the British government took the matter into serious consideration, and in 1857 sent out an expedition (Pallisser's expedition) and declared in 1858 New Caledonia, as it was called under the above mentioned company, an independent colony, to be known in future by the name of British Columbia. At the same time it was urged, that the government of Canada might be empowered to incorporate adjacent portions of land, particularly the so-called Saskatchewan district, east of the Rocky Mountains. This expedition accomplished its chief object, to find a passage across the Rocky Mountains, and also reported favorably in regard to future settlements in the Saskatchewan district, which may be called the intermediate district between the settled portion of British North America and the new gold region in British Columbia. At the same time with Palliser's expedition another expedition was started directly by the Canadian government, and it is our object in the present paper, after having presented a few general remarks on the country, to give a brief synopsis of the course of this latter expedition.

The Saskatchewan district between the Red River and the Rocky Mountains has already, since the beginning of the present century, been the object of many explorations, the most prominent of which are those of Astronomer Thompson†, Lefroy, Richardson, Lord Selkirk, Blodget, and others. They all agree that the Saskatchewan district is well adapted for cultivation. It comprises an immense area, and as early as 1805, Lord Selkirk

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* Vol. xxviii, p. 320.

† Thompson was from 1790 over 30 years in the employ of the Hudson's Bay Company, and the reports of his explorations (37 vols.) are deposited in the Archives of this Company. From fragments of them it appears that Thompson possessed a great knowledge of the country, but it is doubtful whether these reports will ever be accessible to such as are not connected with the Company. Until now the Company has kept them back. [Compare this Journal, xxviii, 344, note.]

said that it could give bread to at least 30 millions of people. In regard to the climate, says Blodget, who is most thoroughly acquainted with the subject, that the average temperature in winter is not below that of St. Petersburg and Moscow; in summer it equals that of northern Italy and New York. The temperature increases, just as in Europe, as you go from east to west. Spring commences at all points almost at the same time. There is no want of rain; grass, forests and buffaloes abound. Useful timber is abundant; coal is found in many places, but particularly rich deposits exist at the foot of the Rocky Mountains, and near the Little Sauris River. The country is level and appears so even, that Blakistone remarked that for the construction of a railroad nothing was required but to put down the rails. Its numerous lakes and rivers can easily be connected for internal communication, and afford even now the only means of transport between the different stations of the Hudson's Bay Company. The Saskatchewan district can also easily be connected with the new gold region by means of commodious roads through Palliser's passage across the Rocky Mountains. This new colony will, by reason of its very favorable situation, its beautiful harbors, but particularly by reason of its wealth in gold, surely rise as speedily as Southern California; and, as it is less capable of agriculture, would naturally become the great market for the products of its eastern neighbors, in the Saskatchewan district.

We may therefore well be justified in prognosticating for this district a prosperous future in regard to agriculture, but we cannot agree with such opinions expressed some time ago in the Montreal Pilot, that by a regularly established road from Lake Superior to Lake of the Woods, Red River, Lake Winnipeg, Saskatchewan river, across the Rocky Mountains to the rivers of British Columbia, thence to the Pacific, all commercial intercourse between Europe and China, Japan and India would take this route. A road which changes so often between land and water can never become a general commercial road for such a distance, not to mention the almost insurmountable difficulties for vessels of a larger draught, such as sudden bends, rapids, falls, shallow waters, etc., and the entirely uncultivated state of the country.

After these few remarks we return to our subject proper. We can give but a brief synopsis, and refer those who desire a detailed account of the Canadian expedition, to the "Reports on the exploration of the country between Lake Superior and the Red River Settlement." A still more minute account is given in the "Papers relative to the Explorations of the Country between Lake Superior and the Red River settlement, presented to both Houses of Parliament, London, 1859." Three charts by

Hind (one a reprint of Thompson's), and a sketch of the regions, which Dawson traveled through, by himself, appeared at the same time.

The members of the Canadian expedition landed July 31, 1857, at Fort William, and started in boats along the usual route of the Hudson's Bay Company for Lake Winnipeg, in order to ascertain the practicability of this route. To this end surveys of rivers were made and a very minute determination of levels. Napier estimates the whole length of the route to be 747 miles, viz: from Lake Superior to Rainy Lake 335 miles; thence to Rat Portage at the northern extremity of Lake of the Woods 176 miles; from this point to Fort Garry on the Red River 236 miles. Of these three portions only the middle one, upon Rainy Lake, which is at an average 460 feet wide and 6 feet deep, forms a continuous water road. Its falls (Chaudiere falls near Fort Francis, 22 feet,) may, according to Dawson, easily be made harmless by two water gates. The two remaining portions of the route can only be travelled by land, unless one prefers the tedious transport from one little river to another. The Kaministiquoria on the first portion of the route cannot be navigated, as its rapids, shallow water places, and falls (Kakabeke falls, 119 feet,) are too numerous. From Little to Great Dog Lake, a distance not over a mile, this river falls 348 feet, and yet the portage in this place has still an elevation of 142 feet over Great Dog Lake. This is the steepest descent on the whole route. The passage upon Dog River is partially obstructed by rocks and sandbanks, and on Prairie Portage, between Lake Superior and Lake Winnipeg, it leads mostly through swamps. The difference of elevation between Lake Superior and Prairie Portage, 54 miles distant from one another, is, after Dawson, 879 feet, according to Napier 887 feet; that between Prairie Portage and Lake Winnipeg (325 miles) is calculated by Dawson 892, by Napier 870 feet. Thus the descent toward the east is much more rapid than toward the west. The canoe route from Savannah River to Rainy Lake has too many portages and the Rivière la Seine is, by reason of the numerous difficulties in its course, entirely objectionable. But the Winnipeg River, from Lake of the Woods to Lake Winnipeg, was by all declared to be the most difficult and impracticable on the whole route. The canoe route on the Pigeon River, from Lake Superior to Rainy Lake along the boundary, is the shortest, but it has 29 portages, of which many lead through United States territory. Another route to the Red River, which is still used by the Hudson Bay Company, commences from Fort York, near Hudson's Bay, and goes up Hays River, through Knee and Holy Lakes, Wepinapanis River, White Water Lake and Sea River, down to Lake Winnipeg; but it requires three weeks of hard work to travel it, besides the ac-

cess to Fort York, through Hudson's Bay is only about two months open during the year. But the most commodious and most frequented road to the Red River over St. Paul and Crow Wing leads entirely through United States territory. In the English possessions the best connection between Lake Superior and the Red River would be established by country roads, the one from Lake Superior to Rainy Lake, the other from Lake of the Woods to the Red River. In regard to the first, however, nothing has as yet been done, and only in the latter district explorations have been made with this view. When Gladman had arrived at Fort Garry (September, 1857,) he sent out engineers Napier and Dawson to reconnoitre this hitherto entirely unknown district, which explorations were continued by Gaudet and Wells during the winter 1857–1858.

The whole country between the Red River and Lake of the Woods appeared perfectly level, although it actually descends toward the east nearly 400 feet. Dry prairies change alternately with wooded districts and extensive swamps, the latter being particularly frequent toward north. The establishment of a road through this district seemed to them an utter impossibility.

Hind went up the Assiniboine River, explored the Great and Little Rat river, examined the valley of the Red river up to Pembina, and followed the Reed Grass or Roseau river up to a great swamp, which separated this stream from a lake of the same name. Unfortunately Hind could not survey this river up to its sources, but all the Indians who lived there agreed that a swamp of 9 miles in extent existed between Roseau lake and Lake of the Woods. This swamp sends the Reed river, 30 miles long, to the latter lake, and another little rapid river, about 40 or 50 miles long, to lake Roseau. From the Great Muskeg morass goes a little river westward into an extensive swamp, from which the Rat river issues.

Gladman was relieved from his post as chief in April, 1858, and Napier also recalled about this time. But Hind went the same spring again with Dickinson, Fleming and Hine on another expedition known as the "Assiniboine and Saskatchewan expedition." Their object was to explore the regions west of the Red River and Lake Winnipeg up to the Saskatchewan river. Before they arrived at Fort Garry, Dawson, Wells and Gaudet had already made some new surveys, around the Red river, Lake Winnipeg, and the lower Assiniboine, and had just left for the lake district. This latter party went by way of Lake Manitobah and Lake Winnipego-sis, over Mossy Portage toward Cedar or Bourbon lake to the grand rapids of the Saskatchewan river. At Mossy Portage they separated; Wells went over Lake Winnipego-sis, Lake Dauphin, Lake Manitobah, the Little Saskatchewan river, which he found to be 8 to 12 feet deep, 250 yards wide,

free from rapids and throughout adapted for steam navigation, thence over Lake Winnipeg to the Red river. The rest of the party followed Swan river to Fort Pelly, and thence went down the Assiniboine river.

Dawson considers the whole alluvial plain east of the Pasquia and Porcupine hills and Dauphin mountains, where the large lakes are situated, well adapted for settlements. It is partly prairie land, for the most part, however, thickly wooded. North of Lake Dauphin wood predominates; south of it the country becomes more open, and toward the Assiniboine an apparently endless prairie commences. Wheat gives abundant harvests near Lake Manitobah and the Little Saskatchewan river, and near the latter even Indian corn may be cultivated. The valley of the Swan river is particularly fertile and its climate equals that of the Red river district. The Red Deer river district has also a good soil and fine climate, as its maple tree forests plainly show. Coal is said to be found in the Porcupine hills and the Duck mountain; Dawson himself found samples of lignite near Snow river. The great alluvial valley of the Assiniboine and its branches will, in his opinion, hereafter become one of the finest wheat growing districts upon earth. Near Moss or Dauphin river, a fine navigable stream, the Indians grow maize, melons and potatoes. Vines, hops, and vetches grow naturally in abundance.

Hind and his companions went (June 14, 1858,) from Fort Garry in a westerly direction over Fort Ellis toward the missionary station near Qu' Appelle lake (July 18), where he divided his corps into three parties: Dickinson traveled on the Qu' Appelle river up to its mouth, thence on horseback to Fort Pelly; Hine surveyed Long lake northwest of the Qu' Appelle mission, then went over land to Fort Pelly to meet Dickinson, and to explore with him the Dauphin mountains; Hind and Fleming followed the Qu' Appelle river up to its source, went over to the elbow of the southern arm of the Saskatchewan or Bow river, on which they travelled down until they reached Fort à la Corne (Aug. 9). The Qu' Appelle and Bow rivers have no connection as Dr. Hector believes. The latter (southern arm of the Saskatchewan) has down from its elbow for a distance of about 100 miles, a width of 300 yards to half a mile, then it becomes narrower and straighter in its course, its sand and mud banks disappear and finally it hurries through a narrow and deep valley, with a strong current toward the northern arm of the Saskatchewan, with which it unites forming one river (Saskatchewan), which now goes toward Fort à la Corne through Pine and Cedar lakes into Lake Winnipeg. Fleming followed this course from Fort à la Corne into Lake Winnipeg, along its western coast, until he reached the Red River. Hind made a

land voyage along Long creek, then turning southeast went over Touch Wood hills to Fort Ellis, where he met Dickinson with whom he returned over White Mud river to Fort Garry (September 4).

But Hind and Fleming soon started on another excursion (September 18). They went in boats along the western shores of Lake Winnipeg, up to the mouth of the Little Saskatchewan, hence (September 29) into Lake Manitobah, and by means of Water Hen river and a lake of the same name reached Lake Winnipego-sis, where they examined the salt springs, which had been imprudently exhausted by the Indians. From here they started for Lake Dauphin, ascended the Dauphin mountains (1700 feet high), and navigated Lake Manitobah in different directions. Hind stayed four days on a little island there, which was much revered by the Indians as the seat of the "Manitou," or fairies. On its northern side were limestone cliffs about fifteen feet high, which by the beating of the waves emitted sounds very similar to chimes from a number of church bells, ringing at a distance. From Oak Point, at the southern extremity of the lake, the party went over land toward Fort Garry, where they arrived the 31st of October, 1858.

Hine, while sojourning on the Red river during the fall months, took photographic views of landscapes, churches, Indians, etc. Dickinson made excursions in the district east of the lower Red River, and in the regions between the Assiniboine and the U. S. boundary, but particularly along Rivière Sal through the Pembina mountains and Blue hills.

Some Canadian journals have blamed this Expedition for not having made any determination of points and for giving generally but little positive information, although $50,000 to $60,000 had been expended for the purpose. They said that the whole country had been much better explored by the late astronomer Thompson. This, however, is an unjust imputation. Astronomical observations of points, although very valuable, cannot be the main object of explorers, who have to run through a great number of districts in a comparatively very short time, and who must give us the general features of the country; moreover, as here a great number of such fixed points already exist, a careful survey of routes by dead reckoning is perfectly sufficient. The reproach that the country had been much better explored by Thompson is most unjust. Thompson's reports were undoubtedly as little accessible to the members of the Canadian expedition as they were to the rest of the world; besides, if we compare Thompson's chart with that of the expedition of 1858, we perceive that our knowledge of the country between Lake Winnipeg and Bow river is more accurate and more complete than Thompson's.

The expedition has achieved much. They made very comprehensive levellings, effected numerous measurements of width, depth and rapidity of rivers and lakes, made geological observations, inquired into the climate, forests, quality of soil, etc., made surveys and discoveries between Lake of the Woods and the Red river, between the Assiniboine river and the U. S. boundary, along the upper Assiniboine and Qu' Appelle rivers, in the district of the great lakes, etc. A comparison of their charts with the older ones of these districts will at once show that the money was not thrown away.

This expedition has moreover excited the curiosity of the people more than that under Capt. Palliser. Thus a society was formed at St. Paul in Minnesota, who, under the direction of Col. Nobles, left this city in June, 1859, with the object to explore the valleys and sources of the Saskatchewan and Columbia rivers. Their plan was, to start from the elbow of Bow river toward the Rocky Mountains, to explore carefully the region of their eastern foot up to Edmonton House, thence to go over Athalaska Portage between Mount Hooker and Mount Brown toward the sources of Thompson's river and here to disperse in different directions. Col. Nobles intended to start for the sources of Columbia river, and to return over Lewis and Clarke's Passage, the Missouri Falls, the valley of the Milk river, Fort Mandan, Big Stone Lake, and Fort Ridgley to St. Paul. Dr. Goodrich accompanies them as physician, and the Smithsonian Institution sent Dr. C. L. Anderson, of Minneapolis, to make scientific observations and collections.

The "Board of Trade" in St. Paul offered a reward of $1000 for the first steamer that should ply on or before the first of June on the Red river, and the "Anson Northup" really commenced her voyages in June. She carries, besides passengers, 100 to 150 tons of cargo, and is intended to do the post service between the mouth of the Shagerme river and Fort Garry, and thus to connect St. Paul, (which sustains a post wagon up to the Shagerme river,) directly with the Red river.

Another company in Canada intend to put four steamers on Rainy lake, Red river and Lake Winnipeg. Even the settlers on the Red river themselves show an active spirit of progress.

ART. XXII.—*Discussion between two Readers of Darwin's Treatise on the Origin of Species, upon its Natural Theology.*

FIRST READER.—Is Darwin's theory atheistic or pantheistic? or, does it tend to atheism or pantheism? Before attempting any solution of this question, permit me to say a few words tending to obtain a definite conception of *necessity*, and *design*, as the sources from which events may originate, each independent of the other; and we shall, perhaps, best attain a clear understanding of each, by the illustration of an example in which simple human designers act upon the physical powers of common matter.

Suppose, then, a square billiard table to be placed with its corners directed to the four cardinal points. Suppose a player standing at the north corner, to strike a red ball directly to the south; his design being to lodge the ball in the south pocket; which design, if not interfered with, must, of course be accomplished. Then suppose another player, standing at the east corner, to direct a white ball to the west corner. This design also, if not interfered with, must be accomplished. Next suppose both players to strike their balls at the same instant, with like forces, in the directions before given. In this case the balls would not pass as before, namely, the red ball to the south, and the white ball to the west, but they must both meet and strike each other in the centre of the table, and, being perfectly elastic, the red ball must pass to the west pocket, and the white ball to the south pocket. We may suppose that the players acted wholly without concert with each other, indeed they may be ignorant of each other's design, or even of each other's existence; still we know that the events must happen as herein described. Now the first half of the course of these two balls is from an impulse, or proceeds from a power, acting from design. Each player has the design of driving his ball across the table in a diagonal line to accomplish its lodgment at the opposite corner of the table. Neither designed that his ball should be deflected from that course and pass to another corner of the table. The *direction* of this second part of the motion, must be referred entirely to *necessity*, which directly interferes with the purpose of him who designed the rectilinear direction. We are not in this case, to go back to find design in the creation of the powers or laws of inertia, and elasticity, after the order of which the deflection, at the instant of collision, necessarily takes place. We know that these powers were inherent in the balls, and were not created to answer this special deflexion. We are required, by the hypothesis, to confine attention in point of time, from the instant preceding the impact of the balls, to the time of their

arrival at the opposite corners of the table. The cues are moved by design. The impacts are acts from design. The first half of the motion of each ball is under the direction of design. We mean by this the particular design of each player. But at the instant of the collision of the balls upon each other, direction from design ceases, and the balls no longer obey the particular designs of the players, the ends or purposes intended by them are not accomplished, but frustrated, by *necessity*, or by the necessary action of the powers of inertia and elasticity, which are inherent in matter, and are not made by any design of a Creator for this special action, or to serve this special purpose, but would have existed in the materials of which the balls were made, although the players had never been born.

I have thus stated, by a simple example in physical action, what is meant by design and what by necessity; and that the latter may exist without any dependence upon the former. If I have given the statement with what may be thought, by some, unnecessary prolixity, I have only to say that I have found many minds to have a great difficulty in conceiving of necessity as acting altogether independent of design.

Let me now trace these principles as sources of action in Darwin's work or theory. Let us see how much there is of design acting to produce a foreseen end, and thus proving a reasoning and self-conscious Creator; and how much of mere blind power acting without rational design, or without a specific purpose or conscious foresight. Mr. Darwin has specified in a most clear and unmistakeable manner the operation of his three great powers, or rather, the three great laws by which the organic power of life, acts in the formation of an eye. (See p. 169). Following the method he has pointed out, we will take a number of animals of the same species, in which the eye is not developed. They may have all the other senses, with the organs of nutrition, circulation, respiration and locomotion. They all have a brain and nerves, and some of these nerves may be sensitive to light; but have no combination of retina, membranes, humors, &c., by which the distinct image of an object may be formed and conveyed by the optic nerve to the cognizance of the internal perception, or the mind. The animal in this case would be merely sensible of the difference between light and darkness. He would have no power of discriminating form, size, shape, or color, the difference of objects, and to gain from these a knowledge of their being useful or hurtful, friends or enemies. Up to this point there is no appearance of *necessity* upon the scene. The billiard balls have not yet struck together, and we will suppose that none of the arguments that may be used to prove, from this organism, thus existing, that it could not have come into form and being without a creator acting to this end with intelligence

and design, are opposed by anything that can be found in Darwin's theory; for so far, Darwin's laws are supposed not to have not come into operation. Give the animals thus organized, food and room, and they may go on, from generation to generation, upon the same organic level. Those individuals that, from natural variation, are born with *light-nerves* a little more sensitive to light than their parents, will cross or interbreed with those who have the same organs a little less sensitive, and thus the mean standard will be kept up without any advancement. If our billiard table were sufficiently extensive, *i. e.*, infinite, the balls rolled from the corners would never meet and the *necessity* which we have supposed to deflect them would never act.

The moment, however, that the want of space or food commences *natural selection* begins. Here the balls meet, and all future action is governed by *necessity*. The best forms, or those nerves most sensitive to light, connected with incipient membranes and humors, for corneas and lenses, are picked out and preserved by natural selection, of necessity. All cannot live and propagate, and it is a necessity, obvious to all, that the weaker must perish, if the theory be true. Working on, in this way, through countless generations, the eye is at last formed in all its beauty and excellence. It must, (always assuming that this theory is true,) result from this combined action of natural variation, the struggle for life, and natural selection, with as much certainty as the balls, after collision, must pass to corners of the table different from those to which they were directed, and so far forth, as the eye is formed by these laws, acting upwards from the nerve merely sensitive to light, we can no more infer design, and from design, a designer, than we can infer design in the direction of the billiard balls after collision. Both are sufficiently accounted for by blind powers acting under a blind necessity. Take away the struggle for life from the one, and the collision of the balls from the other,—and neither of these were designed,—and the animal would have gone on without eyes. The balls would have found the corners of the table to which they were first directed.

While, therefore, it seems to me clear that one who can find no proof of the existence of an intelligent creator, except through the evidence of design in the organic world, can find no evidence of such design in the construction of the eye, if it were constructed under the operation of Darwin's laws; I shall not for one moment contend that these laws are *incompatible* with design and a self-conscious, intelligent creator. Such design, might indeed, have coexisted with the necessity or natural selection; and so the billiard players might have designed the collision of their balls; but neither the formation of the eye, nor the path of the balls after collision, furnishes any sufficient proof of such design in either case.

One, indeed, who believes from revelation or any other cause, in the existence of such a Creator, the fountain and source of all things in heaven above and in the earth beneath, will see in natural variation, the struggle for life and natural selection, only the order or mode, in which this Creator, in his own perfect wisdom, sees fit to act. Happy is he who can thus see and adore. But how many are there who have no such belief from intuition, or faith in revelation; but who have by careful and elaborate search in the physical, and more especially in the organic world, inferred, by induction, the existence of God from what has seemed to them the wonderful adaptation of the different organs and parts of the animal body to its, apparently, designed ends! Imagine a mind of this skeptical character, in all honesty and under its best reason, after finding itself obliged to reject the evidence of revelation, to commence a search after the Creator, in the light of natural theology. He goes through the proof for final cause and design, as given in a summary though clear, plain, and convincing form in the pages of Paley, and the Bridgewater treatises. The eye and the hand, those perfect instruments of optical and mechanical contrivance and adaptation, without the least waste or surplusage;—these, say Paley and Bell, certainly prove a designing maker as much as the palace or the watch prove an architect or a watchmaker. Let this mind, in this state, cross Darwin's work, and find that after a sensitive nerve, or a rudimentary hoof or claw, no design is to be found. From this point upwards the development is the mere necessary result of natural selection; and let him receive this law of natural selection as true, and where does he find himself? Before, he could refer the existence of the eye, for example, only to design, or chance. There was no other alternative. He rejected chance, as impossible. It must then be design. But Darwin brings up another power, namely, natural selection, in place of this impossible chance. This not only may, but, according to Darwin, must of necessity produce an eye. It may indeed coexist with design, but it must exist and act and produce its results, even without design. Will such a mind, under such circumstances, infer the existence of the designer—God—when he can, at the same time, satisfactorily account for the thing produced, by the operation of this natural selection? It seems to me, therefore, perfectly evident that the substitution of natural selection, by necessity, for design in the formation of the organic world, is a step decidedly atheistical. It is in vain to say that Darwin takes the creation of organic life, in its simplest forms, to have been the work of the Deity. In giving up design in these highest and most complex forms of organization, which have always been relied upon as the crowning proof of the existence of an intelligent Creator, without whose intellectual power

they could not have been brought into being; he takes a most decided step to banish a belief in the intelligent action of God from the organic world. The lower organisms will go next.

The atheist will say, wait a little. Some future Darwin will show how the simple forms came *necessarily* from inorganic matter. This is bût another step by which, according to La Place, 'the discoveries of science throw final causes further back.'

SECOND READER.—It is conceded that if the two players in the supposed case were ignorant of each other's presence the designs of both were frustrated, and from necessity. Thus far it is not needful to inquire whether this necessary consequence is an unconditional or a conditioned necessity, nor to require a more definite statement of the meaning attached to the word *necessity* as a supposed third alternative.

But if the players knew of each other's presence, we could not infer from the result that the design of both or of either was frustrated. One of them may have intended to frustrate the other's design, and to effect his own. Or both may have been equally conversant with the properties of the matter and the relation of the forces concerned, (whatever the cause, origin, or nature of these forces and properties), and the result may have been according to the designs of both.

As you admit that they might or might not have designed the collision of their balls and its consequences, the question arises whether there is any way of ascertaining which of the two conceptions we may form about it, is the true one. Now, let it be remarked that *design* can never be *demonstrated.* Witnessing the act does not make known the *design*, as we have seen in the case assumed for the basis of the argument. The word of the actor is not proof; and that source of evidence is excluded from the cases in question. The only way left, and the only possible way in cases where testimony is out of the question, is to infer the design from the result, or from arrangements which strike us as *adapted* or *intended* to produce a certain result, which affords a presumption of design. The strength of this presumption may be zero, or an even chance, as perhaps it is in the assumed case; but the probability of design will increase with the particularity of the act, the speciality of the arrangement or machinery, and with the number of identical or yet more of similar and analogous instances, until it rises to a moral certainty,—*i. e.*, to a conviction which practically we are as unable to resist as we are to deny the cogency of a mathematical demonstration. A single instance, or set of instances, of a comparatively simple arrangement might suffice. For instance, we should not doubt that a pump was designed to raise water by the moving of the handle. Of course the conviction is the stronger, or at least the sooner

arrived at, where we can imitate the arrangement, and ourselves produce the result at will, as we could with the pump, and also with the billiard-balls.

And here I would suggest that your billiard-table with the case of collision, answers well to a machine. In both, a result is produced by indirection,—by applying a force out of line of the ultimate direction. And, as I should feel as confident that a man intended to raise water who was working a pump-handle, as if he was bringing it up in pails-full from below by means of a ladder, so, after due examination of the billiard-table and its appurtenances, I should probably think it likely that the effect of the rebound was expected and intended no less than that of the immediate impulse. And a similar inspection of arrangements and results in nature would raise at least an equal presumption of design.

You allow that the rebound might have been intended, but you require proof that it was. We agree that a single such instance affords no evidence either way. But how would it be if you saw the men doing the same thing over and over? and if they varied it by other arrangements of the balls or of the blow, and these were followed by analogous results? How if you at length discovered a profitable end of the operation, say the winning of a wager? So in the counterpart case of natural selection; must we not infer intention from the arrangements and the results? But I will take another case of the very same sort, though simpler, and better adapted to illustrate natural selection; because the change of direction,—your necessity—acts gradually or successively, instead of abruptly.

Suppose I hit a man standing obliquely in my rear, by throwing forward a crooked stick, called a boomerang. How could he know whether the blow was intentional or not? But suppose I had been known to throw boomerangs before; suppose that, on different occasions, I had before wounded persons by the same, or other indirect and apparently aimless actions; and suppose that an object appeared to be gained in the result, *i. e.*, that definite ends were attained—would it not at length be inferred that my assault, though indirect, or apparently indirect, was designed?

To make the case more nearly parallel with those it is brought to illustrate, you have only to suppose that, although the boomerang thrown by me went forward to a definite place, and at least appeared to subserve a purpose, and the bystanders, after a while, could get traces of the mode or the empirical law of its flight, yet they could not themselves do anything with it. It was quite beyond their power to use it. Would they doubt, or deny *my* intention, on that account? No: they would insist that design on my part must be presumed from the nature of the results;—that, though design *may* have been wanting in any one

case, yet the repetition of the result, and from different positions and under varied circumstances, showed that there *must* have been design.

Moreover, in the way your case is stated, it seems to concede the most important half of the question, and so affords a presumption for the rest, on the side of design. For you seem to assume an actor, a designer, accomplishing his design in the first instance. You—a bystander—infer that the player effected his design in sending the first ball to the pocket before him. You infer this from observation alone. Must you not from a continuance of the same observation equally infer a common design of the two players in the complex result, or a design of one of them to frustrate the design of the other? If you grant a designing actor, the presumption of design is as strong, or upon continued observation of instances soon becomes as strong, in regard to the deflection of the balls, or variation of the species, as it was for the result of the first impulse or for the production of the original animal, &c.

But in the case to be illustrated, we do not see the player. We see only the movement of the balls. Now, if the contrivances and adaptations referred to (p. 229,) really do "prove a designer as much as the palace or the watch prove an architect or a watchmaker,"—as Paley and Bell argue, and as your skeptic admits, while the alternative is between design and chance,—then they prove it with all the proof the case is susceptible of, and with complete conviction. For we cannot doubt that the watch had a watchmaker. And if they prove it on the supposition that the unseen operator acted *immediately*,—*i. e.*, that the player directly impelled the balls in the directions we see them moving, I insist that this proof is not impaired by our ascertaining that he acted *mediately*, *i. e.*, that the present state or form of the plants or animals, like the present position of the billiard-balls, resulted from the collision of the individuals with one another, or with the surroundings. The original impulse, which we supposed was in the line of the observed movement, only proves to have been in a different direction; but the series of movements took place with a series of results, each and all of them none the less determined, none the less designed.

Wherefore, when, at the close, you quote Laplace, that "the discoveries of science throw final causes farther back," the most you can mean is, that they constrain us to look farther back for the impulse. They do not at all throw *the argument for design* farther back, in the sense of furnishing evidence or presumption that only the primary impulse was designed, and that all the rest followed from chance or necessity.

Evidence of design, I think you will allow, every where is drawn from the observation of adaptations and of results, and

has really nothing to do with any thing else, except where you can take the *word* for the *will.* And in that case you have not *argument for design*, but *testimony*. In nature we have no testimony; but the argument is overwhelming.

Now, note that the argument of the olden time,—that of Paley, &c., which your skeptic found so convincing,—was always the argument for design in the movement of the balls *after deflection.* For it was drawn from animals produced by generation, not by creation, and through a long succession of generations or deflections. Wherefore, if the argument for design is perfect in the case of an animal derived from a long succession of individuals as nearly alike as offspring is generally like parents and grand-parents, and if this argument is not weakened when a variation, or series of variations, has occurred in the course, as great as any variations we know of among domestic cattle, how then is it weakened by the supposition, or by the likelihood, that the variations have been twice or thrice as great as we formerly supposed, or because the variations have been 'picked out,' and a few of them preserved as breeders of still other variations, by natural selection?

Finally let it be noted that your element of *necessity*, has to do, so far as we know, only with the picking out and preserving of certain changing forms, *i. e.*, with the natural selection. This selection, you may say, must happen under the circumstances. This is a necessary result of the collision of the balls; and these results can be predicted. If the balls strike so and so, they will be deflected so and so. But the *variation* itself is of the nature of an origination. It answers well to the original impulse of the balls, or to a series of such impulses. We cannot predict what particular new variation will occur from any observation of the past. Just as the first impulse was given to the balls at a point out of sight, so the impulse which resulted in the variety or new form was given at a point beyond observation, and is equally mysterious or unaccountable, except on the supposition of an ordaining will. The parent had not the peculiarity of the variety, the progeny has. Between the two is the dim or obscure region of the formation of a new individual, in some unknown part of which, and in some wholly unknown way, the difference is intercalated. To introduce necessity here is gratuitous and unscientific; but here you must have it to make your argument valid.

I agree that judging from the past—it is not improbable that variation itself may be hereafter shown to result from physical causes. When it is so shown you may extend your necessity into this region, but not till then. But the whole course of scientific discovery goes to assure us that the discovery of the cause of variation will be only a resolution of variation into two

factors,—one, the immediate secondary cause of the changes, which so far explains them; the other an unresolved or unexplained phenomenon, which will then stand just where the product, variation, stands now, only that it will be one step nearer to the efficient cause.

This line of argument appears to me so convincing, that I am bound to suppose that it does not meet your case. Although you introduced players to illustrate what design is, it is probable that you did not intend, and would not accept, the parallel which your supposed case suggested. When you say that the proof of design in the eye and the hand, as given by Paley and Bell, was convincing, you mean, of course, that it was convincing, so long as the question was between *design* and *chance*, but that now another alternative is offered, one which obviates the force of those arguments, and may account for the actual results without design. I do not clearly apprehend this third alternative.

Will you be so good, then, as to state the grounds upon which you conclude that the supposed proof of design from the eye, or the hand, as it stood before Darwin's theory was promulgated, would be invalidated by the admission of this new theory.

First Reader.—As I have ever found you, in controversy, meeting the array of your opponent, fairly and directly, without any attempt to strike the body of his argument through an unguarded joint in the phraseology, I was somewhat surprised at the course taken in your answer to my statement on Darwin's theory. You there seem to suppose that I instanced the action of the billiard balls and players as a parallel, throughout, to the formation of the organic world. Had it occurred to me that such an application might be supposed to follow, legitimately, from my introduction of this action, I should certainly have stated that I did not intend, and should by no means accede to, that construction. My purpose in bringing the billiard table upon the scene was to illustrate, by example, *design* and *necessity*, as different and independent sources from which results, it might indeed be identical results, may be derived. All the conclusions therefore that you have arrived at through this misconception or misapplication of my illustration, I cannot take as an answer to the matter stated or intended to be stated by me. Again, following this misconception, you suppose the skeptic (instanced by me as revealing through the evidence of design, exhibited in the structure of the eye, for its designer, God,) as bringing to the examination a belief in the existence of design in the construction of the animals as they existed up to the moment when the eye was, according to my supposition, added to the heart, stomach, brain, &c. By skeptic I, of course, intended one who doubted the existence of design in every organic struc-

ture, or at least required proof of such design. Now as the watch may be instanced as a more complete exhibition of design than a flint knife or an hour-glass; I selected, after the example of Paley, the eye, as exhibiting by its complex but harmonious arrangements a higher evidence of design and the designer, than is to be found in a nerve sensitive to light, or any mere rudimentary part or organ. I could not mean by skeptic one who believed in design so far as a claw, or a nerve sensitive to light was concerned, but doubted all above. For one who believes in design at all will not fail to recognize it in a hand or an eye. But I need not extend these remarks, as you acknowledge in the sequel to your argument that you may not have have suited it to the case as I had stated it.

You now request me to "state the grounds upon which I conclude that the supposed proof of design from the eye and the hand, as it stood before Darwin's theory was promulgated, is invalidated by the admission of that theory." It seems to me that a sufficient answer to this question has already been made in the last part of my former paper; but as you request it I will go over the leading points as there given with more minuteness of detail.

Let us then suppose a skeptic, one who is yet considering and doubting of the existence of God, having already concluded that the testimony from any and all revelation is insufficient, and having rejected what is called the *a priori* arguments brought forward in natural theology, and pertinaciously insisted upon by Dr. Clark and others, turning as a last resource to the argument from design in the organic world. Voltaire tells him that a palace could not exist without an architect to design it. Dr. Paley tells him that a watch proves the design of a watchmaker. He thinks this very reasonable, and although he sees a difference between the works of nature and those of mere human art, yet if he can find in any organic body, or part of a body, the same adaptation to its use that he finds in a watch, this truth will go very far towards proving, if it is not entirely conclusive, that in making it, the powers of life by which it grew were directed by an intelligent, reasoning master. Under the guidance of Paley he takes an eye, which, although an optical, and not a mechanical, instrument like the watch, is as well adapted to testify to design. He sees, first that the eye is transparent, when every other part of the body is opaque. Was this the result of a mere Epicurean or Lucretian "fortuitous concourse" of living "atoms?" He is not yet certain it might not be so. Next he sees that it is spherical and that this convex form alone is capable of changing the direction of the light which proceeds from a distant body, and of collecting it so as to form a distinct image within its globe. Next he sees at the exact place where this image must

be formed a curtain of nerve work, ready to receive and convey it, or excite from it, in its own mysterious way, an idea of it in the mind. Last of all, he comes to the crystalline lens. Now he has before learned that without this lens an eye would by the aqueous and vitreous humors alone form an image upon the retina, but this image would be indistinct from the light not being sufficiently refracted, and likewise from having a colored fringe round its edges. This last effect is attributable to the refrangibility of light, that is, to some of the colors being more refracted than others. He likewise knows that more than a hundred years ago Mr. Dollond having found out, after many experiments, that some kinds of glass have the power of dispersing light, for each degree of its refraction, much more than other kinds, and that on the discovery of this fact, he contrived to make telescopes in which he passed the light through two object-glasses successively, one of which he made of crown and one of flint glass, so ground and adapted to each other that the greater dispersion produced by the substance of one should be corrected by the smaller dispersion of the other. This contrivance corrected entirely the colored images which had rendered all previous telescopes very imperfect. He finds in this invention all the elements of design, as it appeared in the thought and action of a human designer. First, conjecture of certain laws or facts in optics. Then, experiment proving these laws or facts. Then, the contrivance and formation of an instrument by which those laws or facts must produce a certain, sought, result.

Thus enlightened, our skeptic turns to his crystalline lens to see if he can discover the work of a Dollond in this. Here he finds that an eye, having a crystalline lens placed between the humors, not only refracts the light more than it would be refracted by the humors alone, but that in this combination of humors and lens, the colors are as completly corrected as in the combination of Dollond's telescope. Can it be that there was no design, no designer, directing the powers of life in the formation of this wonderful organ? Our skeptic is aware that in the arts of man, great aid has been, sometimes, given by chance, that is, by the artist or workman observing some fortuitous combination, form, or action around him. He has heard it said that the chance arrangement of two pairs of spectacles, in the shop of a Dutch optician, gave the direction for constructing the first telescope. Possibly, in time, say a few geological ages, it might in some optician's shop, have brought about a combination of flint and crown glass which, together, should have been achromatic. But the space between the humors of the eye is not an optician's shop where object-glasses of all kinds, shapes, and sizes are placed by chance, in all manner of relations and positions. On the hypothesis under which our skeptic is making his examina-

tion,—the eye having been completed in all but the formation of the lens,—the place which the lens occupies when completed, was filled with parts of the humors and plane membrane, homogeneous in texture and surface, presenting, therefore, neither the variety of the materials, nor forms which are contained in the optician's shop for chance to make its combinations with. How then could it be cast of a combination not before used, and fashioned to a shape different from that before known, and placed in exact combination with all the parts before enumerated, with many others not even mentioned? He sees no parallelism of condition then, by which chance could act in forming a crystalline lens, which answers to the condition of an optician's shop, where it might be possible in many ages for chance to combine existing forms into an achromatic object-glass.

Considering, therefore, the eye thus completed and placed in in its bony case and provided with its muscles, its lids, its tear-ducts, and all its other elaborate and curious appendages, and, a thousand times more wonderful still, without being encumbered with a single superfluous or useless part, can he say that this could be the work of chance? The improbability of this is so great, and consequently the evidence of design is so strong, that he is about to seal his verdict in favor of design when he opens Mr. Darwin's book.

There he finds that an eye is no more than a vital aggregation or growth, directed, not by design nor chance, but moulded by natural variation and natural selection, through which it must, necessarily, have been developed and formed. Particles or atoms being aggregated by the blind powers of life, must become under the given conditions, by natural variation and natural selection, eyes, without design, as certainly as the red billiard ball went to the west pocket, by the powers of inertia and elasticity, without the design of the hand that put in motion. (See Darwin, p. 169.)

Let us lay before our skeptic the way in which we may suppose that Darwin would trace the operation of life, or the vital force conforming to these laws. In doing this we need not go through with the formation of the several membranes, humors, &c., but take the crystalline lens as the most curious and nicely arranged and adapted of all the parts, and as giving moreover a close parallel, in the end produced, to that produced by design, by a human designer, Dollond, in forming his achromatic object-glass. If it can be shown that natural variation and natural selection were capable of forming the crystalline lens, it will not be denied that they were capable of forming the iris, the sclerotica, the aqueous humors, or any and all the other parts. Suppose, then, that we have a number of animals, with eyes yet wanting the crystalline. In this state the animals can see, but

dimly and imperfectly, as a man sees after having been couched. Some of the offspring of these animals have, by *natural variation*, merely, a portion of the membrane which separates the aqueous from the vitreous humor, a little thickened in its middle part, a little swelled out. This refracts the light a little more than it would be refracted by a membrane in which no such swelling existed, and not only so, but in combination with the humors, it corrects the errors of dispersion and makes the image somewhat more colorless. All the young animals that have this swelled membrane see more distinctly than their parents or brethren. They, therefore, have an advantage over them in the *struggle for life*. They can obtain food more easily; can find their prey, and escape from their enemies with greater facility than their kindred. This thickening and rounding of the membrane goes on from generation to generation by natural variation; natural selection all the while "picking out with unerring skill all the improvements, through countless generations," until at length it is found that the membrane has become a perfect crystalline lens. Now where is the design in all this? The membrane was not thickened and rounded to the end that the image should be more distinct and colorless; but, being thickened and rounded by the operation of natural variation, *inherent* in generation, natural selection *of necessity* produced the result that we have seen. The same result was thus produced *of necessity*, in the eye, that Dollond came at, in the telescope, with design, through painful guessing, reasoning, experimenting, and forming.

Suppose our skeptic to believe in all this power of natural selection; will he now seal up his verdict for design, with the same confidence that he would before he heard of Darwin? If not, then "the supposed proof from design is invalidated by Darwin's theory."

SECOND READER.—Waiving incidental points and looking only to the gist of the question, I remark that, the argument for design as against chance in the formation of the eye, is most convincingly stated by you on p. 235–237. Upon this and numerous similar arguments the whole question we are arguing turns. So, if the skeptic was about to seal his verdict in favor of design, and a designer, when Darwin's book appeared, why should his verdict now be changed or withheld? All the facts about the eye, which convinced him that the organ was designed, remain just as they were. His conviction was not produced through testimony or eye-witness, but design was irresistibly inferred from the evidence of contrivance in the eye itself.

Now, if the eye as it is, or has become, so convincingly argued design, why not each particular step or part of this result?

If the production of a perfect crystalline lens in the eye—you know not how,—as much indicated design, as did the production of a Dollond achromatic lens,—you understand how—then why does not "the swelling out" of a particular portion of the membrane behind the iris—caused you know not how—which, by "correcting the errors of dispersion and making the image somewhat more colorless," enabled the "young animals to see more distinctly than their parents or brethren," equally indicate design—if not as much as a perfect crystalline, or a Dollond compound lens, yet as much as a common spectacle glass?

Darwin only assures you that what you may have thought was done directly and at once, was done indirectly and successively. But you freely admit that indirection and succession do not invalidate design, and also that Paley and all the natural theologians drew the arguments which convinced your skeptic wholly from eyes indirectly or naturally produced.

Recall a woman of a past generation and show her a web of cloth; ask her how it was made, and she will say that the wool or cotton was carded, spun, and woven by hand. When you tell her it was not made by manual labor, that probably no hand has touched the materials throughout the process, it is possible that she might at first regard your statement as tantamount to the assertion that the cloth was made without design. If she did, she would not credit your statement. If you patiently explained to her the theory of carding machines, spinning jennys, and power-looms, would her reception of your explanation weaken her conviction that the cloth was the result of design? It is certain that she would believe in design as firmly as before, and that this belief would be attended by a higher conception and reverent admiration of a wisdom, skill, and power so greatly beyond any thing she had previously conceived possible.

Wherefore, we may insist that, for all that yet appears, the argument for design, as presented by the natural theologians, is just as good now, if we accept Darwin's theory, as it was before that theory was promulgated; and that the skeptical Juryman, who was about to join the other eleven in an unanimous verdict in favor of design, finds no good excuse for keeping the Court longer waiting.

ART. XXIII.—*Description of three New Meteoric Irons, from Nelson County, Ky., Marshall County, Ky., and Madison County, North Carolina;* by J. LAWRENCE SMITH, M.D., Prof. of Chemistry, University of Louisville, Ky.

Nelson County, (Ky.) Meteorite.—This came into my possession about two months ago, being obtained from a ploughed field where it may have laid for a considerable length of time, attention was drawn to it by a plough striking it; its metallic character leading the neighboring farmer to believe it to be silver.

It is a flattened mass of tough metal, a little scaly at one corner, being 17 inches long, 15 inches broad, and 7 inches in the thickest part, shelving off like the back of a turtle, and weighs 161 lbs.

It is free from any large proportion of thick rust, consequently showing no indications of chlorine. On analysis, the following constituents were found in 100 parts, No. 1 in the table below:

	(1.)	(2.)	(3.)
Iron, - - - -	93·10	90·12	91·12
Nickel, - - - -	6·11	8·72	7·82
Cobalt, - - - -	·41	·32	·43
Phosphorus, - - -	·05	·10	·08
Copper, - - - -	*trace*	*trace*	*trace*
	99·67	99·26	99·45

Marshall County, (Ky.) Meteorite.—A piece of this Meteorite was sent to me from Marshall County, in this State. I have not yet seen the entire mass, which is said to weigh 15 lbs., and to be scaly in structure. It has the usual characteristics of meteoric iron, as seen from the analysis, No. 2.

Madison County, (N. C.) Meteorite.—This meteorite was presented to me some time ago by the Hon. T. L. Clingman, of North Carolina. It came from Jewel Hill, Madison County, of that State. There is a great deal of thick rust on the surface, with constant deliquescence from chlorid of iron. Its form and surface indicates that it is entire, its dimensions are 7×6×3 inches, with a number of indentations; its weight is 8 lb. 13 oz. Its composition is given in the analysis, No. 3.

ART. XXIV.—*Description of a new Trilobite from the Potsdam Sandstone;* by FRANK H. BRADLEY, with a note by E. BILLINGS.

[Read before the Am. Assoc. for the Advancement of Science, at Newport.]

CONOCEPHALITES MINUTUS, (n. sp.)

1. 2. 3.

Fig. 1. The head magnified. The dotted lines represent the supposed outlines of the parts not preserved.
Fig. 2. The pygidium magnified.
Fig. 3. A detached cheek, magnified.

Cephalic shield apparently semi-circular, or nearly so; anterior margin as far as preserved with a narrow slightly elevated rim, just within which there is a rather strong groove. Glabella conical, slightly narrowed at the neck segment, three-fourths the whole length of the head, very convex and obtusely carinated along the median line. Neck segment rounded and prominent; neck furrow narrow, but well defined. There are two pairs of deep glabellar furrows which are inclined inwards and backwards at an angle of about 45°; their inner extremities distant from each other rather more than one-third the width of the glabella. The anterior lobe is a little less than one-half the whole length of the glabella, excluding the neck segment; the two posterior pairs are nearly equal to each other. The glabella is distinctly separated from the cheeks by a narrow, deep groove, which extends all round. From the anterior lobe on each side a narrow filiform ridge curves outwards and backwards on the fixed cheek to the edge of the portion preserved. The eyes appear to be situated just where these ridges terminate as represented in figure 1. Judging from the portion of the eye preserved in a detached cheek-plate, its form is semi-annular, and its length at least one-fourth that of the glabella. Its distance must be at least one-half the width of the glabella. Caudal shield nearly as large as the head, its width scarcely equal to half its length; the lobes nearly equal; the middle lobe very convex with five sharp transverse grooves; the side lobes somewhat flat, and each with five grooves.

The largest head discovered is exactly two lines in length.

The course of the facial suture has not been ascertained. The surface of the glabella in one of the specimens appears to be smooth, but in none of the others can it be distinguished.

The specimens are mostly in a clayey layer, which is full of fragments of all degrees of perfection; in one specimen I count ten heads and three tails, all in a fair state of preservation. In two instances, I have found the casts of maxillary plates, showing distinctly the elevated margin, of one of which I give a figure.

The original specimens were collected, (at High Bridge, near Keeseville, N. Y.,) in August, 1856, while on a geological excursion with Col. Jewett of Albany, but were not recognized until July, 1857, when a second visit to the locality secured a few *casts* in the solid sandstone, none of the clay layer being obtained. By the kindness of Prof. Dana, the specimens were presented at the Am. Assoc. for Adv. of Sci. at Montreal, but were not recognized as belonging to any known species. Since that time, I have looked for descriptions, but cannot find any to correspond.

At the same locality, I also procured the cast, a Pleurotomaria, and one of what seems to have been a plate from the stem of a crinoid.

New Haven, June 15th, 1860.

Note by E. Billings.—Mr. Bradley having favored me with a view of his very interesting specimens, I think there can be no doubt but that they belong to the genus *Conocephalites.* If this reference be correct, then we have at least three, if not four species in North America.

1. *C. antiquatus* (Salter,) described from "a cast in a brown sandstone, said to be a bouldered fragment from Georgia." (See Quart. Jour. Geol. Soc., vol. xv, p. 554.)

2. *C. minutus* (Bradley.) In this species, the form of the glabella and its proportions in relation to the length of the head are almost precisely the same as in *C. antiquatus*, and yet I think the two are not identical, for the following reasons: In the first place, all the specimens of *C. minutus* are of a nearly uniform size, the length of the head being about two lines, and, therefore, it seems probable that they are the remains of adult individuals. The total length would thus be about half an inch, while Mr. Salter's species is full one inch and three-fourths. In the second place, the distance of the eye from the glabella, in *C. antiquatus* is only one-third the width of the glabella, but in *C. minutus* it must be at least one-half the width. These are the only differences that can be well made out, from the imperfect specimens, but they seem to me sufficient to indicate two species. Mr. Salter says further, that the lobes of the glabella in *C. antiquatus* are very obscure, and that the ocular ridge, if any existed, must have been very slight. His specimen was somewhat abraded. In *C. minutus* the ocular ridge is, for so small a species, very strongly defined, and the glabellar furrows are so deep

that it would require a very considerable amount of abrasion to obliterate them.

3. *C. Zenkeri*, (n. sp.) This is a new species recently discovered in the magnesian limestone near Quebec. It will probably be described in the next No. of the Canadian Naturalist and Geologist.

4. There is in the collection of the geological survey of Canada, a plaster cast of the surface of a fragment of rock which holds four specimens of a trilobite, each about the size of *C. antiquatus*. They appear to me to belong to the genus *Conocephalites*. The original specimen was collected in Newfoundland, in the same slate that holds *Paradoxides Bennettii* (Salter,) and I am informed that it is in the possession of a gentleman who lives somewhere in the United States, but whose name or place of residence, I have not been able to ascertain.

Of the above four species, Mr. Bradley's is at present the most important as it fixes indisputably, at least one point in the geological range of the genus on this side of the Atlantic. In Europe, *Conocephalites* has not been found out of the *primordial zone* of Barrande, but the Quebec and Keeseville specimens show that here it reaches the Lower Silurian.

Montreal, July 25th, 1860.

ART. XXV.—*On the Combustion of Wet Fuel, in the Furnace of Moses Thompson;* by B. SILLIMAN, Jr., Prof. Gen. and App. Chem. in Yale College.

[Read before the Am. Assoc. for the Adv. of Sci., at Newport, August, 1860.]

IN all ordinary modes of combustion, it is well known that the use of wet fuel is attended with a very great loss of heat, rendered latent in the conversion of water into steam. As the most perfectly air dried wood still contains about 25 per centum of water, according to the experiments of Rumford, the term *wet fuel* might seem appropriate to all fuels, but mineral coal and charcoal. But technically, this term is restricted to substances like peat and those residual products of the arts which, like spent tan, begasse and dye stuffs contain at least one half and often more than half of their weight of water. Until a recent period the attempt to consume these products as sources of heat has been attended with uneconomical results, or total failure. It is the object of this paper to describe a mode of combustion in which by a modification in the form of the furnace the combustion of wet fuel is not only rendered consistent with the best economical results; but which as it involves chemical reactions never before, it is believed, successfully applied for such

purposes, is deserving of particular notice from a scientific as well as from a practical point of view.

It is a well established fact in chemistry, that the affinity of carbon for oxygen, at high temperatures is so strong, that if oxygen is not present in a free state, any compound containing oxygen, which happens to be present is decomposed, in order to satisfy this affinity. This fact is well illustrated in the familiar case of the Blast Furnace where this affinity is employed to deprive the ores of iron of their oxygen in the process of reduction to metallic iron.

In the first stages of combustion, in wet fuels, the chief products given off are steam from the drying of the wet mass, smoke or volatilized carbon and oxyd of carbon, with, of course, a variable proportion of carbonic acid and carburetted hydrogen. These products in all ordinary furnaces, pass on together into the stack, carrying with them the heat which they have absorbed and rendered latent. The problem presented is then to recover the heat thus locked up and lost, and by the furnace now under consideration *this is accomplished by shutting off almost entirely the access of the outer air and causing the wet fuel to supply its own supporter of combustion drawn from the decomposition of the vapor of water at a high temperature by its reaction with free carbon and the oxyd of carbon.*

The practical solution of this problem was first successfully accomplished, as appears from a decision of Patent Commissioner Holt, by the late Moses Thompson, in 1854. The controversial questions growing out of this invention, are entirely foreign to our present purpose and in no way affect its practical or scientific value. Suffice it to say, in passing, that we find in this invention another instance of a truth already so often signalized in the history of inventions, that important results are often obtained, of the highest value in promoting material prosperity and the welfare of society, by those who are guided in their search only by the result in view, and not by any exact knowledge of the scientific principles involved.

Mr. Thompson seems to have been inspired with the conviction that if he could bring the products from the combustion of wet fuel together in a place, hot enough for the purpose, and from which the atmospheric air was excluded, they would, as he expresses it in his patent, mutually "*consume each other.*" This notion was realized, and the reaction secured between the elements of water and the carbon of smoke, or the oxyd of carbon in a part of the furnace called by the inventor, the *mixing chamber.*

Wherever that place may be situated, or however constructed, the one essential thing about it, is, that it should be a very hot place, and one to which the atmospheric air can have no direct access, until it has passed by, and through the burning fuel. It

is in fact a retort or place for combination and reaction, and may be a distinct chamber or flue, or only a recess or enlargement greater or less of the main furnace. Wherever it may be placed, or however built, it must meet the essential conditions of a high temperature, and of atmospheric isolation. In this mixing chamber, then, the important chemical reaction before insisted on, must be set up. The vapor of water is decomposed, furnishing its oxygen to the highly heated carbon to form carbonic acid, while the oxyd of carbon is in like manner exalted to the same condition, and any excess of carbon forms with free hydrogen, marsh gas or light carburetted hydrogen. The vapor of water is thus made to give up not only its constituent elements to form new compounds with oxygen, producing in the change great heat, but a great part of the heat absorbed by the water in becoming steam is also liberated in this change of its physical and chemical condition. Moreover as all these products of combustion and of chemical reaction pass together over the bridge-wall of the furnace into a space from which atmospheric air is not excluded, it then and there happens that any free hydrogen, light carburetted hydrogen or oxyd of carbon which have previously escaped combustion, take fire and burn, yielding up their quota of heat to the general aggregate.

Such is the intensity of heat in that portion of the furnace where these reactions take place that only the most solid structures of refractory fire bricks will endure it, and the color seen throughout that portion of the furnace is of the purest white.

In view of the facts already stated it is easy to understand why it is that when the reactions described are once set up, the admission of a free current of atmospheric air should immediately check the energy of the combustion and soon result in total suspension of the peculiar energy of this furnace. The air containing only one-fifth part of its bulk of oxygen gas, the active agent in combustion, the access of so large a proportion of cold air—four-fifths of which are not only indifferent but positively prejudicial from the quantity of heat it absorbs,—it happens that the temperature of the mixing chamber is rapidly reduced below the point at which carbon can decompose vapor of water and the instant that point is reached the arrival of fresh supplies of steam completes the decline of energy and the furnace commences forthwith to belch forth from its stack dense volumes of smoke and watery vapor. When in proper action not a particle of smoke is visible from the stack of a furnace in which wet fuel is burning, and what is more remarkable the reactions are so evenly balanced that no wreaths of watery vapor are observed, while in the earlier stages of combustion before the proper temperature in the mixing chamber is reached, both these products are seen in great abundance.

DESCRIPTION OF THE FURNACES.

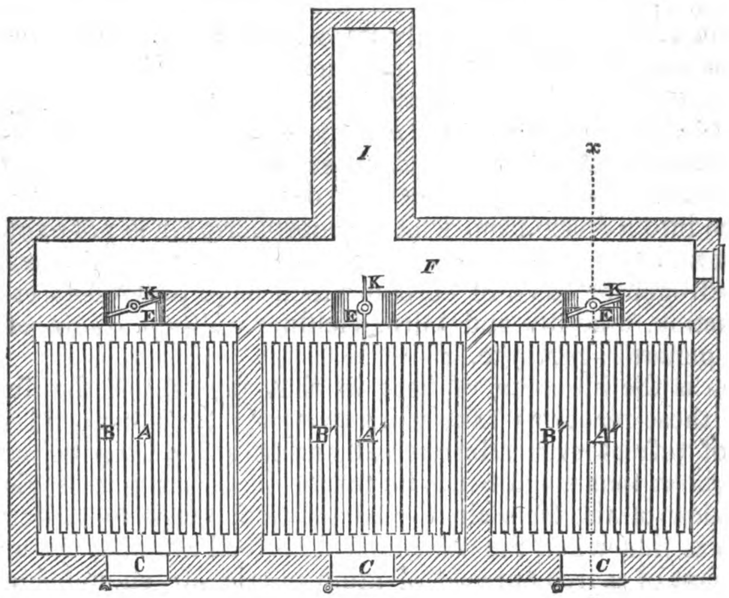
1st. Furnace for combustion of wet tan, sawdust, &c.

Fig. 1, is a horizontal section of a furnace constructed according to the specifications of Thompson's first patent, (issued April 10th, 1855).

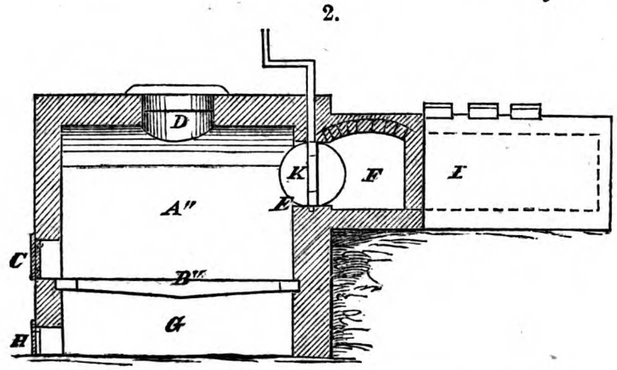
Fig. 2, is a vertical section of the same in the line *x y*, of figure 1.

Similar letters indicate corresponding parts in both figures.

1.



2.



The furnace shown in these figures has three square or oblong fire chambers, A, A', A'', side by side, experience having shown that not less than three compartments are required to secure the best results in the practical working of the furnace, although in some cases two may suffice, but frequently more than three are

desirable. The fire chambers are furnished with grate bottoms *B*, *B'*, *B''*, of fire bricks, and are arched at top. Each chamber has a door *C*, in front for lighting and tending the fire. This opening is seldom used after the furnace is once set in action. The wet fuel is supplied through the opening *D* at top. *E* is an opening at the back of each chamber leading to the flue *F*, or the mixing chamber. This opening may be provided with a damper *K* (which must be of fire clay, if of iron the intense heat soon destroys it). Each chamber has a separate ash pit *G* with its opening *H*. This although called an ash pit serves a most important purpose in the economy of the furnace as a receptacle for the burning coals which constantly fall into it from the lower part of the wet mass above, as will be more particularly explained beyond.

If the furnace is used for generating steam the best place for the boiler is over the flue *I*. The inventor remarks in his first patent that the current from the mixing chamber in passing to the place of use, in the case of burning wet tan or other very wet fuel, should descend or pass under a bridge to the place of use equal to about one half of the depth of the burning chamber between the grate and the crown, then rise to the place of use. In case of dry or nearly dry fuel, such as green wood and saw dust, the current should rise immediately after leaving the burning chamber to the place of use.

The mode of conducting the operation of the furnace is as follows: fires being lighted in all the fire chambers with dry fuel and the masonry heated to a high degree, two of the three chambers *A A'* are fed with wet fuel and have their ash pits closed. The other fire chamber is kept in action by dry fuel (its ash pit door being proportionally open) until the process of combustion sets in over the surface of the pile of wet fuel resting on the grates of the other furnaces. As soon as this is the case, wet fuel is added by degrees to the third fire chamber, the ash pit door being at the same time closed. If things have been properly managed so far, the process will now continue by the addition of new portions of wet fuel to each furnace in succession or alternately. The temperature of the mixing chamber *F* is now seen to be of the most perfect whiteness and not a visible particle of smoke issues from the stack.

Before discussing this process more in detail, let us first consider the Inventor's description of his furnace as designed more particularly for the consumption of *begasse* or crushed cane stalks.

2. *Furnace for Combustion of Wet Cane Begasse.*

Fig. 3 is a sectional side view, the interior and exterior form of the furnace, and its several parts according to the specifications of Thompson's patent of Dec. 15, 1857.

3.

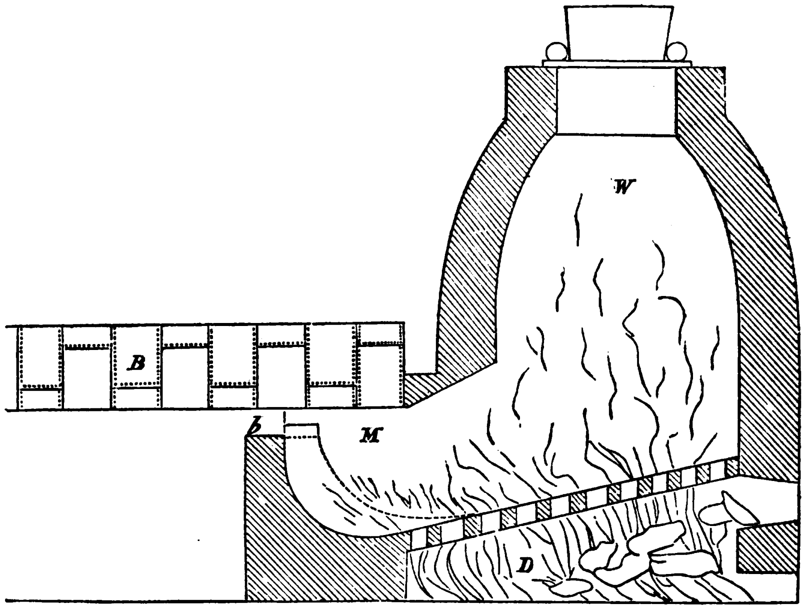


Fig. 4 is a front sectional view of the same, showing the combination of two double furnaces.

4.

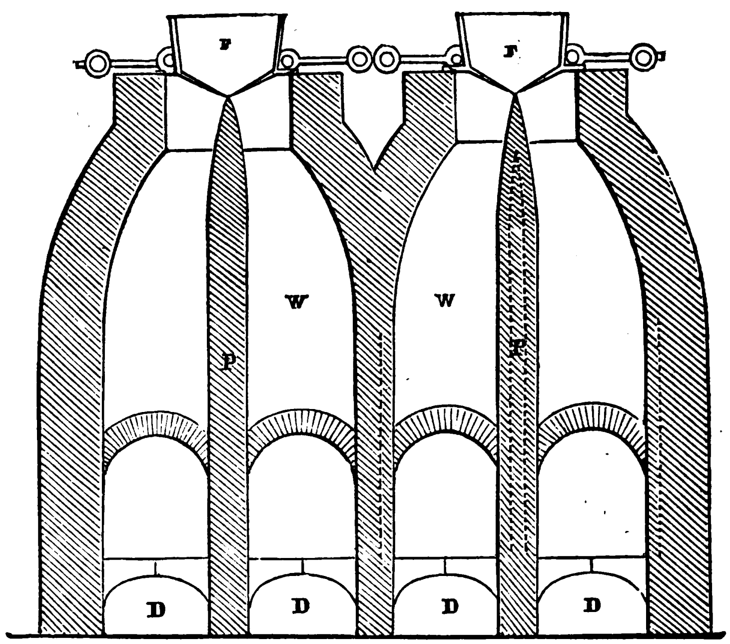
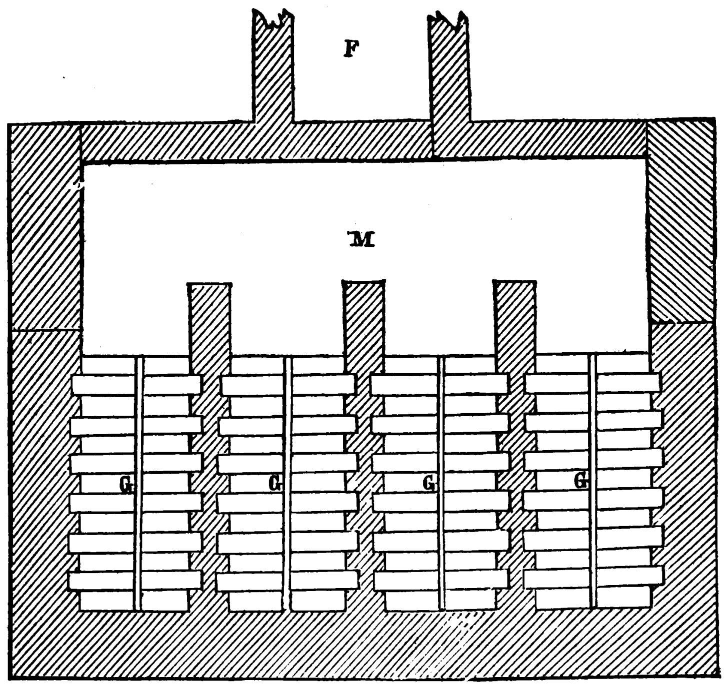


Fig. 5 is a horizontal view of the grate and its relation to the mixing chamber *M* and flue *F*.

5.



Here let the Inventor speak for himself in the language of the patent last named.

"I build two furnaces side by side, each nearly square in its horizontal section. Towards the top I draw in the wall in such manner as to form a kind of dome with a sufficient opening at top to feed the bagasse. The outer walls of these furnaces should be from 24 to 30 inches thick and built with a special view to rendering them non-conducting, the wall near the top, and the partition between the two furnaces may be thinner. In each furnace chamber there should be a partition of fire brick extending across it from front to back and rising nearly to the top, dividing it into two nearly equal parts. The whole interior of the furnace should be of fire brick. The main chamber of each furnace should be divided into two parts—upper and lower—by a fire brick grate about one-fifth the height of the furnace above the hearth, the back end of the grate being a little lower than the front. The bottom of the lower chamber may be a grate with an ash pit, but a hearth is much better.

In each furnace at the front, on each side of the central partition and immediately under the front end of the grate should be doors for feeding wood or other dry fuel, and directly under these doors at the hearth of the lower chamber should be draught openings capable of adjustment to support combustion in the lower chamber.

Extending across the back of both furnaces, and opening into both by flues is a mixing chamber into which all the gases from both furnaces enter in a highly heated state and mix and consume each other on their way to the boiler and stack. This chamber should be about one-half the

capacity of all the fire chambers and it should extend down about as low as the back end of the grate. The flue through which the products of combustion pass out of this chamber and under the boiler should be in section about one square foot to forty cubic feet of mixing chamber.

The feed openings at the top of the furnaces should be closed by doors which open inwards by the weight of the feed, but are self-closing, and do not yield to pressure from within.

The sides of the interior of the upper or wet fuel chamber or drying chamber of the furnace, except the front and back, are corrugated up and down, as also the sides of the central walls or partitions as shown by the dotted lines in Fig. 4, the corrugations extending down to the grate; these corrugations are for the purpose of allowing the heat to radiate upwards from the fire chamber for heating the masonry, and the wet charge, while the gases or vapors driven out of the wet charge by the heat are allowed to descend to the fire chamber or the mixing chamber. If the surfaces of this masonry were smooth the bagasse would lie against them in such a manner as to obstruct the upward radiation of the heat and the downward passage of the vapors.

These corrugations are unnecessary in burning tan and sawdust.

The spaces between the grate bars for burning bagasse should be about 6 inches wide for the finest grinding and twenty inches for the coarsest, and should vary between these widths according to the fineness of grinding, but for sawdust and tan much less, say from one inch to $\frac{3}{4}$ of an inch. The grate should be made of fire brick.

The operation of my furnace is as follows: A hot fire of dry fuel is kindled in the lower or fire chambers of the furnaces and after it has been continued till the masonry is well heated, the chamber above the grate is fed with the begasse or other wet fuel. This hot fire in the fire chamber, especially towards the front of it under the principal mass of the wet fuel, must be preserved throughout the operation. The heat from the masonry and the fire chamber will be communicated to the wet fuel which will cause steam and other gases to issue from it and mix with the intensely hot gases of combustion from the fire chamber, and in a short time the mixing chamber will present intense combustion and heat, the dampers of the fire chambers being partially closed. The lower part of the wet charge will by degrees become dry and charred and will fall through the grate prepared as above unto the fire chamber and supply or nearly supply the place of other dry fuel in preserving the fire in this chamber and the wet fuel being from time to time supplied will furnish in a highly heated state aqueous vapors which descending through the corrugations and otherwise into the fire chamber and mixing chamber, will be decomposed, furnishing much oxygen to the fire, and supply the oxygen necessary to combustion of all the combustible gases issuing from the fire chamber. If by accident the fire in the lower part of the furnace should predominate, the draught should be diminished and more wet fuel added, and, if by accident, the fire in the fire chamber should become too much cooled down the draught should be let on, and any deficiency of dry fuel should be supplied to the fire chamber. Under proper management little or no dry fuel need be fed to the fire chamber after the operation is fairly commenced, the charred matter falling through the open grate will supply its place; and the caloric thus produced by the combustion of wet

fuel, will be vastly greater than from the same quantity by measure of the same fuel when dry. In the fire chamber and in the mixing chamber under intense heat the carbonaceous gases will decompose the steam from the wet fuel and effect complete combustion.

When the operation is fairly commenced if the water in the wet charge amounts to say fifty per cent by weight of the fuel, the dampers of the fire chamber should be nearly or quite closed to exclude the air; vapor from the wet charge will then descend through the corrugations and otherwise into the fire chambers and support the combustion therein, while other portions of the vapor will enter the mixing chamber and complete the combustion there. If the fuel, however, contains much smaller quantities of water, more air in proportion should be admitted at the damper, the object being to admit no more air than will supply the deficiency of the vapor.

In the drawings, *D* represents the chambers for the dry fuel, *W* those for the wet, *M* the mixing chamber, the dotted line *m* in Fig. 3 limits it for the wettest bagasse, P the partition, F the feed openings for the wet fuel with their doors, B the boiler, *b* the bridge. Little if any of the boiler should extend over the mixing chamber. If any considerable portion of the mixing chamber is covered by the boiler its cooling influence will prevent the decomposition of the the vapor and defeat the object of my invention. Great care should be observed in giving proper dimensions to the mixing chamber, for the perfection of the combustion and the efficiency of the furnace depend greatly upon it. The principal object of this chamber is to give the combustible carbonaceous gases from the fire, and the aqueous gases from the mass of wet fuel an opportunity of mingling together in such a manner and under such circumstances that the aqueous vapor will be decomposed by the carbonaceous gases, and its oxygen given out to complete the combustion of the carbon, without the introduction of air into the mixing chamber, thus saving the caloric previously communicated to the wet charge, while drying it and charring its lower portions, and avoiding the cooling influences of cold air. This can take place effectually only in the presence of a high degree of heat and in the absence of a supply of free oxygen. If this chamber be too small to receive these gases as fast as the furnace is able to produce them the operation will of course be choked and impeded. If the chamber is larger than can be kept densely filled with these gases, of course atmospheric air will be found there at the commencement, and will continue to find its way into the chamber, and while atmospheric air is present, the carbonaceous gases will take their oxygen from that source principally instead of decomposing the steam, and the heat in the chamber will be much diminished and the large quantity of nitrogen $\frac{4}{5}$ contained in the air, which is neither a combustible nor a supporter of combustion, will at once greatly increase the volume of gases to be sent forward to the stack and proportionably decrease its temperature; and when the chamber becomes very large the cooling influences become so great that combustion will immediately cease, and smoke mingled with steam oxygen and nitrogen, will go forward, thus wasting the fuel and imparting only a faint degree of heat to the boiler.

I have therefore fixed the size of the mixing chamber by many careful experiments—and that given above will produce the desired effect with

wet bagasse. For dryer fuels furnishing less vapor, the mixing chamber should be proportionably increased in size to supply the defficiency with air and to effect complete combustion. Rules more precise would be inconsistent with the nature of the subject.

A large and hot fire should always be preserved in the fire chamber below the grate, and directly under the charge of wet fuel, for the purpose of driving the vapor out of it and charring its lower portion—and the grate is left much more open than in furnaces for burning dry fuel of the same size, for the purpose of allowing the charred portions of the wet charge to fall through to supply fuel for this fire as fast as it becomes fit for that purpose, thus consuming the mass with little or no expenditure of other fuel.

What I claim as my improvement in furnaces for burning bagasse and and other fuels too wet to be conveniently burned in the usual way and well known ways is:

First, the combination of two chambers, the one above the other, and separated by a grate, the lower one for the combustion of any known dry carbonaceous fuel, and the upper one in immediate proximity therewith to receive heat therefrom for heating and drying the charge of wet fuel, with a mixing chamber, into which both continuously and simultaneously discharge their gases before reaching the thing to be heated, for mingling and mutual combustion.

I also claim in combination with said fire chamber and wet fuel chamber or drying chamber making the grate upon which the wet charge rests sufficiently open to allow the lower portion of the wet charge as it becomes dried and charred to fall through into the fire chamber and keep a hot fire therein, supplying the place of other dry fuel, while the uncharred portions of the wet fuel is properly supported by the grate till dried as described.

I also claim placing the mixing chamber of combustion in substantially the same position described relatively to the fire, and the wet charge, so that the products of combustion from the dry fuel may pass along the lower part of the wet charge, drying and charring it on their way to the mixing chamber, and reach it without being in any considerable degree obstructed or cooled by the wet charge substantially as shown.

I wish it distinctly understood that I make no claim to any of the parts or combination above specified except in their application to the preparation and combustion of wet fuels."

It will be observed that in this mode of combustion the wet fuel is subject to a constant process of distillation by the fire in the ash pit. The products of this distillation react on each other in the mixing chamber in the manner already described, while at the same time a portion of watery vapor is decomposed in the ash pit.

Theoretically no more heat can be generated in this mode of combustion than is consumed in the transformation of water into steam and the conversion of fixed into volatile products. But it is by no means a matter of indifference whether the oxygen requisite for complete combustion is drawn from the atmosphere or is derived from the decomposition of water by carbon

and its oxyd. In the former case, not only is there a great loss of heat carried away by the inefficient nitrogen of the air, but the diluted oxygen can never produce so intense a heat with the carbon as is the result of the reaction of the nascent oxygen with that element. Although Mr. Thompson was no chemist, he did not fail with his natural acumen to perceive this advantage and in his earliest patent he remarks: "After ample experiments I have discovered that any results that can be produced, by the use of dry fuel are inferior (to those obtained from my process) in proportion to the quantity used, and that results like mine can only be obtained by the use of wet fuel * * * fed into an intensely heated chamber: under such circumstances the water in the fuel in presence of the carbonaceous substances in the furnace will be decomposed, giving its oxygen to the carbonaceous matter, dispensing with a draft and its cooling and wastful influence and rendering the combustion so perfect that no smoke is visible."

Although this mode of combustion of wet fuel is now in use on many sugar plantations in Louisiana, and in some Tanneries of Pennsylvania and New York, no notice of it has so far as I am aware appeared in the scientific Journals. I am not without personal experience of its operation on a large scale, having in 1857 enjoyed the opportunity of studying carefully the management of one of Thompson's furnaces in three compartments (similar to Figs. 1 and 2) built for the combustion of wet peat. That fuel contained over seventy-five per cent of its whole weight of water and was too wet for the best results. But with the use of one-fourth part of dry wood, even this extremely wet and otherwise valueless fuel was rendered efficient, three cords (of 128 cubic feet) of wet peat and one cord of dry wood doing the work of four cords of dry wood in driving a steam boiler.

ART. XXVI.—*Note on a case of Artificial Crystallization of Metallic Copper and Dinoxyd of Copper;* by J. W. MALLET.

A FLASK, in which nitric oxyd had been prepared from nitric acid and scraps of copper, was allowed to remain over night by the pneumatic trough—the end of the gas-delivering tube dipping under the surface of the water. On the next day several scraps of copper were observed dotted over with very minute but brilliant crystals of metallic copper, which under the microscope proved to be octahedrons and combinations of the octahedron with the cube and dodecahedron, of various sizes—the largest measuring about twelve-hundredths of a millimeter along an octahedral edge. Along with these crystals of copper there were little cubes of the dinoxyd of copper in great abundance—

the latter not more than two or three hundredths of a millimeter on the edge, translucent, and of a splendid garnet red color.

On examination it became evident that these two bodies had been deposited in the crystalline state in consequence of the formation of the so-called "Bucholzian circuit"—one solid and two liquids so arranged as to produce electric action. As the flask cooled down the water from the peneumatic trough gradually rose in the tube, and in time ran down into the flask. The latter being in an inclined position the water flowed gently down the neck and formed a distinct stratum above the strong solution of nitrate of copper. Some of the scraps of metal which had projected above the surface of the solution, were now immersed, partly in the latter and partly in water, and it was upon the lower portion of each of these scraps—the part immersed in solution of nitrate of copper—that crystallization had taken place.

The deposition of metallic copper under similar conditions was observed by Bucholz, and other metals may, as is well known, be thus crystallized from solutions of their salts. The crystallization of the dinoxyd of copper was effected by Becquerel, but the arrangement of substances in his experiment was different from that now noticed—he placed a strip of copper in a saturated solution of the nitrate, the lower end of the strip touching some protoxyd of copper at the bottom of the solution. The crystals which he obtained were octahedral, not cubic.

Art. XXVII.—*Review of Dr. Antisell's Work on Photogenic Oils, &c.*

[Concluded from page 121.]

In describing the methods of purification proposed by Selligue, we shall make no attempt to follow their various details, our limited space compelling us to content ourselves with only the broadest generalities. Selligue sets forth at length two methods: 1st. A cold treatment which consists in agitating the oil with sulphuric, muriatic, or nitric acid. This agitation should be thorough, he says, and should be continued for a longer or shorter time according to the nature and quantity of the matter treated. Here follows a description of his agitators. After several hours' repose, the oil may be decanted, except from muriatic acid in which case more time and a larger amount of acid is required. After the oil has been thus separated from the deposit of tar, the acid remaining in it must be neutralized by means of an alkali. I prefer, says Selligue, to employ the lye of soap-boilers marking 36° to 38° [B.?], since it is easy of application, and produces a sure effect; I thus precipitate together the coloring matter and tar which would otherwise have remained in the oil. The oil is then decanted: if it is the first distillation of the crude oil I do not allow the mixture to subside entirely, preferring to leave a portion of the alkali mixed with the oil, and to distil off only $\frac{3}{4}$ths of the latter. * * * When the soda lye—in quantity slightly greater than is necessary to neutralize the acid—is added, the liquid must be agitated violently in order that each particle of the oil may be brought in contact with the alkali; this agitation must be continued until the color of the oil undergoes change.

The oil becomes less odorous and less highly colored after each such "cold treatment."

After having been allowed to separate from the lye, the oil is decanted off; if it has not lost much of its color the process has been badly conducted. It should be stated that the oil must not be agitated several times with the alkali, for, by so doing, the dark color of the oil would be restored. * * * As for the residues of the soda treatment, continues Selligue, they should be allowed to stand at rest during some days beneath a portion of oil, which will protect them from contact with the air; the clear lye at the bottom being then drawn off may be used for other operations, while the remainder is a soap, containing excess of alkali. By adding to it a little grease a soap can be made, or by adding water, grease may be separated. This grease is similar to that used for wagons.

2d. A warm treatment which follows the cold, and consists of a series of fractional distillations,—special operations for the purification of the "light-stuffs," being resorted to. For the details of these we must refer to the original specification of Selligue—a truly classical document which should be read by every one interested in the manufacture of coal-oils.* Nor will our limited space permit us to cite the detailed "example" of his treatment which Selligue has described. We trust that we have already written enough to enable the reader to judge whether or no Selligue understood his business.

As for paraffine, Selligue obtained it by subjecting the oil to a low temperature in order that this substance might crystallize. The mixed oil and paraffine was then thrown upon fine metallic filters through which the oil flowed while the paraffine was separated. Or one may separate, he says, the oil by imbibition, but this occasions a great loss of oil and also requires more labor. * * With this specification the scientific discussion of the subject by Selligue appears to have ceased, yet in the same year he replies† to a note published by Chenot‡ who asserted that the oil of shale often contains arsenic, denying that arsenic can be found in the products from his own establishments. He again describes the locality and geological position of his shale, the method of distillation employed,—how the temperature is gradually elevated, &c.

This is of interest as showing that the manufacture of coal-oil in France was no ephemeral fancy, but for many years was a well established branch of industry. In this connection the scientific research, upon the commercial products of the distillation of bituminous shale, of Saint-Evre§ should also be mentioned. Contemporaneous with Selligue we find other inventors occupied with the same subject. Thus Holthorp,‖ in 1841, claims that he has first discovered a means of purifying the fluid substance, which he calls "schiste," resulting from the distillation of coal or of bitumen. His attention was evidently chiefly devoted to the volatile naphthas, but he also obtained paraffine.

Guillard Meynier,¶ in 1842, speaks of the fixed oil from shale, telling us that it may be used for lighting or lubricating and that paraffine may be separated when the oil is cooled or treated with alcohol.

In the same year Bonnet** in treating of liquids suitable for lighting incidentally mentions eupion and paraffine.

Nor should we omit to mention the very interesting article upon ***Hydrocarbures Liquides***, by A. Mallet (in Laboulaye's Dictionnaire des Arts et Manufactures, 2d Ed., Paris, 1854††), in which Selligue's processes are incidentally

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* A tolerably accurate English translation of this important patent may be found in the specification of M. A. B. B. Du Buisson, 1845; specification No. 10,726 of the English Patent Office.

† *Comptes Rendus*, 1845, xx, 573.

‡ *Ibid*, xx, 306.

§ *Comptes Rendus*, 1849, xxix, 339.

‖ *Brevets d'Invention*, liii, 263.

¶ *Brevets d'Invention*, lxxviii, 91.

** *Ibid*, lxxix, 63.

†† A portion of this article, which directly refers for the most part only to the volatile products suitable for "burning fluids," which may be obtained in any way from coal, is also contained in Dingler's *Polytechnisches Journal*, 1847, cvi, 128.

described. After discussing in detail the light volatile products obtained by distilling coal-tar, he says, we have still to speak of the carbo-hydrogens from shales; a branch of industry which we owe entirely to Selligue—cut off, alas! prematurely, in the midst of his career so full of discoveries and of useful works. As is well known, he obtained by distilling shales from the environs of Autun: I, volatile ethereal oils, II, fixed oils, III, oils combined with paraffine from which he prepared grease for carriages, IV, paraffine suitable for making candles, &c. Among all these bodies, Mallet continues, we have only to occupy ourselves with the volatile oils. Further on M. remarks that the acid and alkaline treatment used by Selligue is similar to that proposed by Barral for products from coal-tar. Thus far, says Mallet, these hydro-carbons have found no application,* partly on account of their insupportable odor when not purified and partly on account of their high price—about $10·00 the hundred lbs.—when purified.

We have been at no pains to ascertain whether the industrial distillation of shales, so well grounded by Selligue, has been continued in France without interruption up to the present time, for we know of no reason to doubt the fact. Certain it is that coal-oils produced by French manufactories were exhibited, at the Exposition Universelle at Paris in 1855, and likewise in 1851 at London.†

To any one familiar with the extreme slowness with which the practical applications of chemistry are even now imparted to, and recorded by, scientific writers, it would have been no matter of surprise if the results obtained by Selligue had remained uncopied upon the records of the French patent office. Such however was not the case. From the preceding citations it will be seen that his results were published in various well known journals and were widely diffused. Dumas, in his *Traité de Chimie Appliquée aux Arts*,‡ expressly calls attention to them. They are also noticed in the *Handwörterbuch der reinen und angewandten Chemie, von* Liebig, Poggendorff u. Wœhler, 1844, iii. 364. What we cannot explain is the apparent ignorance of these facts which was exhibited by several of the leading chemists of Great Britain on the occasion, of a trial,§ Young, *v.* White and others held in June, 1854, in the Court of Queen's Bench before Lord Chief Justice Campbell.

Several patents for the production of oils [coal-oils] from bituminous substances were meanwhile obtained in England. Butler,‖ for example, in describing his "improvements in the manufacture of oil and gas" proposes to distil bituminous shales, &c. for the purpose of obtaining oil and gas free from naphthaline. The shale, best after wetting it with water if the principal object is to obtain oil, is distilled in common gas retorts under which a gentle fire is lighted. As soon as oil begins to flow over freely the fire is to be increased and the retorts brought to a red heat; a large quantity of gas is thus obtained which is collected in a gas holder. The rough oils, as Butler informs us, may be purified by washing with sulphuric acid, filtration, &c., or they may be used in the rough state for making oil-gas. The oils in their rough state are often found entirely free from oxygen, and if obtained by the process described never contain so much as is contained in the coal-tar obtained in the coal-gas works where the coal is thrown into retorts already brought to a red heat. These oils in their rough state are further distinguished from coal-tar by their containing no naphthaline. Moreover the less volatile part of the

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* It will be observed by the reader of Mallet's treatise that he is interested only in a single branch of the subject, viz., the volatile naphthas—"light stuffs," just as we are here giving prominence to another portion of it, viz., the fixed, or paraffine-oil; and that he holds the naphthas from shale in small repute, since in his opinion they can never compete in the matter of cost with those from coal-tar.

† A. U. Morean (No. 1361, Cat. 9), *Bas-Rhin.*

‡ Paris, 1844, t. vii, p. 390; also t. iii, p. 315 of the Liege edition; and B. vii, S. 510 of the German translation.

§ Reported in Barlow's London Journ. of Gas Lighting, Aug. 10, 1854, vol. iii, p. 508.

‖ Patent granted Jan. 29, 1833. Specification No. 6375 of the English Patent Office.

oil [No. 2] offers another characteristic feature; if after being drawn off and distilled, and if in this latter process the more volatile or first proceeds, say one half of the quantity acted upon, be set apart and the remaining half exposed to a low temperature, there will soon appear in this part of the distilled oil small flakes of a white, odorless, and light substance which is a compound of carbon and hydrogen [paraffine]. The familiarity with the subject, somewhat remarkable in view of the early date of his patent, which Butler exhibits cannot fail to strike the reader. This inventor was however unfortunate in the idea of trying to make at the same time oil and gas—in endeavoring to reconcile two antagonistic processes.

In 1841, Sept. 4, Count de Hompesch of Prussia* specified certain "improvements in obtaining oils and other products from bituminous matters."† It is well known, he says, that oils may be obtained from these substances but from the imperfection of the processes now used the quantity obtained is small, the quality inferior, and the smell noxious. My invention consists in an improved process, whereby I increase the quantity, improve the quality, and remove or greatly modify the smell. I have found by experiment, he continues, that the oil from shale, &c., possesses three different characters which may be called essential oil, intermediary fat oil, and thick oil, and these oils I separate by means of peculiar apparatus—which he describes in detail. In distilling shales heat is applied until the temperature reaches 100° R. = 257° F., at which temperature essential oil will pass over. The charge, after having been subjected to this temperature for half an hour, is pushed forward in the retort which is now subjected to a heat of 200° R. = 482° F., by which increased heat the intermediary or fat oil is obtained. After having subjected the charge to this increased temperature for half an hour the workmen again pushes the charge further on in the retort where it becomes of a red heat; the vapor now given off yields the thick oil. The carbonization is now complete; and I obtain these three separate oils by the gradual increase of the heat; and I effect this distillation without decomposition of the substance, the vapors escaping from the retort as fast as they are formed.

The essential oil is separated from the fat oil by exposing the mixture to a current of steam by which the more volatile oil is carried off. The oil [fixed] thus prepared must be filtered and is then ready for application to all kinds of machinery, being very fat, works without friction and leaves no sediment. The essential oil is collected and subjected to further treatment.

The specification of Du Buisson‡ for improvement in the distillation of bituminous substances, is an almost literal translation of Selligue's last patent. Indeed, Du Buisson tells us that the extensive works at Autun, Department of the Saone and Loire, France, are partly his property and that he has the management of them as chemist. He affirms moreover that the most important results have there been attained — results which place the distillation and treatment of schistus among the most useful and productive of chemical manufactures.

Since we have already extracted largely from Selligue's specification it is unnecessary to cite more of it here. It is a little curious that this most important patent is not mentioned in Dr. Antisell's "list of English Patents" (p. 141).

The well known attempts to prepare paraffine and oils from peat§ need not be discussed here. Another patent, not mentioned in Dr. A.'s list, is that of

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* Specification No. 9060 of the English Patent Office.

† In a "memorandum of alteration," dated July 5, 1842, de Hompesch claims the right of distilling "bituminous schists, shales, or slates, or other rocks or minerals containing bitumen or bituminous substances."

‡ Dated June 23, 1845. Specification No. 10,726 of the English Patent Office.

§ Antisell, p. 85; compare Rees Reece's patent dated Jan. 23, 1849. Specification 12,436 of the English Patent Office.

George Michiels.* It is peculiarly interesting since a portion of it relates to the preparation of oils from caking coals. Michiels proposes in fact to prepare coke from bituminous coals, and from mixtures of such coals with anthracite, by moistening the powdered coal with water and introducing it—in charges of six tons—into brick retorts furnished with ordinary condensing apparatus and other appliances. The retorts are then heated as if it were intended to produce gas, with this difference, that the temperature for the first fifty hours should not exceed nascent red heat, or 964° F.; after that time it should be increased progressively until it attains a clear red heat, which would be about the ninety-sixth hour, I should remark, continues Michiels, that about the sixtieth hour I shut off the communication between the retort and the condensor by closing the hydraulic valves, and at the same time open the valve on top of the retort, &c., so as to allow the air to enter, which burns the hydrocarburets [now being evolved] and the products of that combustion heat the retort, &c. in passing through the flues which surround the retort. * * * I thus obtain coke, ammoniacal liquors and liquid hydro-carburets. These "hydro-carburets" were repeatedly distilled by M. in order to obtain as much light volatile oil as possible, A heavy yellow oil of density 0·911, or lower, was also obtained which according to M. will be found applicable to many useful purposes, and is suitable for his principle object of turning into gas.

Further on (p. 15 of his specification) Michiels explains that this oil is well adapted for manufacturing gas upon a small scale, since the gas prepared from it requires no purification, and since it can be used in any of the ordinary apparatus for making gas from camphene, oil, or resin. In a word, he proposed using it just as rosin oil is now so largely employed by private gas-works in this country, or as Boscary and Butler had used the same coal-oil before him.

We pass† to a consideration of the well known labors of Mr. James Young of Glasgow.‡ From evidence brought forward in the trial already cited it appears that Mr. Young's attention was called in 1847 to a mineral oil [petroleum] found exuding from a coal pit at Riddings in Derbyshire. From it he obtained a good lubricating oil which he continued to prepare as long as his supply of petroleum lasted. Occupied as he was with the subject it can surprise no one that he should soon have turned his attention to the distillation of the highly bituminous mineral of Torbane-hill, now known as Boghead coal in England and in this country, which was introduced to public notice in 1850.§ From this substance Young was enabled to prepare a much larger amount of oil per ton of mineral than had been obtained by any of his predecessors. To the discovery of the vast source of an admirable raw material which the Boghead mine furnished is evidently due the immense increase in the production, and of course consumption, of coal-oil which immediately ensued. To this we say, more than to anything else is to be attributed the rise and progress, during the past few years, of the almost innumerable manufactories of coal-oil on the continent of Europe and in our own country. From the impetus thus given, a branch of industry which had long been, comparatively speaking, of only local importance soon attained an enormous development.‖

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* Granted April 30, 1850. Specification No. 13,066 of the English Patent Office.

† Making no pretence, be it understood, that we have been able to collect all that has been published upon the subject before 1850.

‡ Patent dated Oct. 7, 1850.

§ According to Mr. T. G. Barlow, *London Journal of Gas Lighting*, iii, 519.

‖ We cannot, in this connection, forbear quoting the following pertinent remarks from Lord Campbell's charge to the jury in the case—Young *v.* White and others (see *London Journal of Gas Lighting*, iii, 521)—already cited.

"And this brings me to an observation," says his Lordship, "which I meant to make, and which I should have been sorry if I had forgotton, which is this—that it was the discovery of this Boghead coal that seems to have given the great value

Let it be distinctly understood that we would in no wise detract from the real merit of Mr. Young. Uniting, as he does, no small share of chemical knowledge with the cautious, untiring energy of his countrymen, few men could have been found better qualified to grasp the golden opportunity of which he so fortunately availed himself. His name must ever remain associated with those of the distinguished observers from whose labors this most important branch of industry has resulted. It is Dr. Antisell only whom we blame for his incorrect and partial "history." When, for example (on p. 14), Dr. A. tells us that: "only since the year 1850 has the manufacture of paraffine from pit-coal, turf and bituminous shales succeeded as an art. The first manufacture was that of James Young in Manchester, by whose process, from 100 parts of Cannel-coal 40 per cent of oil and 10 per cent of paraffine could be obtained." He makes a statement which is grossly exaggerated—if not *entirely* at variance with fact—as our readers must already have perceived.

We willingly quote what follows: "In thus showing [*i. e.*, dogmatically *asserting*] that the practical manufacture of oils from coal is due to James Young, it may not be amiss to call attention to what it was which he produced from coals by distillation. He claimed the production of paraffine oils—not the production of naphtha or benzule [benzol?], nor naphthalin, but paraffine and its congeners: this involves the slower distillation of coals at a lower temperature than had been hitherto effected, and this novelty in practice was followed by the novel result of a copious production of isomeric liquid hydrocarbons; so that really two great results were first demonstrated, practically by the operation of Young's process, namely—1st, That coal was a material from which liquids could be manufactured economically, as tar, bitumens, and schists had been hitherto employed; and 2nd, That the liquids so formed were paraffine-containing compounds." Having merely to suggest that the sentence might have been more tersely put. For in truth it means only—if it means anything—that in the opinion of Dr. A., Mr. Young was the first person who distilled coal [on a manufacturing scale?] at comparatively low temperatures. What Dr. Antisell's private views regarding "low temperature" or "practical" may be, we are ignorant. But we do know that when, 30 years ago, Reichenbach distilled quantities of coal of 75 lbs. weight each, and exercised the greatest care in maintaining the temperature of his retort at as low a degree as was admissible, as he has most minutely described in the memoirs which we have already cited;—when he obtained paraffine and eupion as results of his operation; he most certainly demonstrated the practicability and the manner of preparing both paraffine and "paraffine-oil."

All this however does not appear to satisfy Dr. A. in the least degree, who repeatedly assures his readers that the manufacture of oil from *coal* dates from the patent of Mr. Young. Since our author has seen fit to dwell at length upon this point and to devote so much space to its discussion we may be pardoned for referring to it here.

As is well known the term "coal" is applied in common language to a great variety of mineral combustibles no two kinds of which are precisely alike while some sorts are exceedingly unlike others. The term is at best merely conventional; used, in lack of any better one, to designate substances with the real nature of which we are still almost entirely ignorant.

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to paraffine, because until then I do not find it was obtained in such quantities as really were of any considerable value; but the Boghead coal now being discovered, and this schist or coal being discovered, which is of very rich quality, and having a great deal of what is the essential part of the paraffine; from that time it has become much more important; and that may explain why, although the mode of obtaining paraffine was before well known, it should not have been put in practice because it would not appear that it could be put into practice with much profit or benefit, unless you had such a substance as Boghead coal on which you could operate."

In confirmation of this view compare also: Payen, *Précis de Chimie Industrielle*, 4e Ed.. Paris, Hàtchette. 1859. t. ii, p. 685.

With the flint-like anthracite of Wales, the beautiful Albert coal*—but a step removed from asphaltum—of Hillsboro, New Brunswick; our common lignites, frequently shading into peat, and the bituminous shales as frequently passing by insensible gradations into common slate, as points upon its extreme limit, we have within the circumference of the circle an infinite number of substances, shading into each other by scarcely perceptible degrees,—all of which are, in technological language at least, varieties of *coal*. The "amplitude of variation" which this species, or rather this term, enjoys is indeed so great that it would be a matter of no small difficulty to choose any single member of the medley as a central point, or even to conceive of an ideal coal to which all other varieties should be referred. In attempting any such selection a native of one of our sea-board states would assuredly lean towards anthracite; the South-German towards his excellent lignites; the Scotchman towards his cannel; while Newcastle would claim a proverbial right of precedent. We would, for our own part, vote for the last named, or some other good caking coal, capable of furnishing both gas and serviceable coke, and of being used for an infinite variety of purposes. Starting from this then as a type, observe, that as we pass towards the cannels, the different varieties of coal become better and better suited for the manufacture of gas or oil, *i. e.*, they contain more and more hydrogenous compounds. The appearance of the mineral meanwhile approaching more and more closely to that of slate, while at the same time the value of the fixed carbonaceous residue becomes less and less, soon ceasing to be "coke" at all, but rather a more or less carbonaceous slate. At length a maximum of hydrogenous matter is reached as in the case of Boghead coal, a slaty substance, the fixed residue from the distillation of which is a slightly carbonaceous stone, valueless as fuel and useful only, as a substitute for bone-black, for purposes of disinfection or decolorizing liquids, uses to which the residues of the French shales have long been applied. Beyond this maximum, as the amount of gas and oil-producing substances diminishes, and the amount of earthy matter increases,—taking the place of the fixed carbon in our typical caking coal, we pass into "bituminous shales," and these become less and less bituminous until at length we reach common clay slate containing no organic matter whatsoever. We have here traced no fancy sketch. That the "cannel coals" thus gradually pass into "bituminous shales" is now well enough known, at least to gas engineers and other practical observers. It would not for that matter be exceedingly difficult to obtain a continuous series of specimens exhibiting this almost insensible gradation. Now did Mr. Young devote his attention to the distillation of caking coals similar to our typical Newcastle? By no means! On the contrary we find him occupied with a mineral which was called indifferently "shale" or "coal," until it was in 1853 decided in a Scottish court that it should henceforth be legally known as coal.†

* On page 18 of Dr. A.'s work the following remark occurs. "In one respect they [bitumens] differ from coal. In no case can an organic tissue or structure be demonstrated when they are examined under the microscope. Viewed in this light the mineral found at the Albert mine, New Brunswick, should be classed as a bitumen since Dr. J. Leidy was unable to detect any trace of structure in its mass." We cannot understand the motive of our author in thus again dragging to light this negative result, for it is perfectly well known to the scientific men of America that Prof. J. Bacon detected the existence of vegetable structure in the interior of masses of the Hillsboro coal. See *Reports on the Geological Relations, chemical analyses, and microscopic examination of the coal of the Albert Coal Mining Co., situated in Hillsboro, Albert Co., N. B.*, by Chas. T. Jackson. M.D. New York: printed by Nesbitt, 1851, p. 30; compare this Journal, [2], xiii, 276.

† We had supposed, when the above was written, that the decision of this court had been sustained. That in this we were mistaken appears from the following, which we extract from a statement in a late number of the *London Journal of Gas*

We would cast no reflection upon the judgment rendered in this famous suit. Looking at it as a mere matter of equity, depending upon the business relations of the parties at issue, this verdict was in our own opinion, just. But the fact of this mineral being, or not being, called a *coal*, does not in the least degree prevent it from being *also a shale*; and that it is more nearly related to the shales than to the coals is believed by a large proportion of those who are intimately acquainted with it, be they scientific or practical men. We have no space to discuss at greater length this quibble of Dr. Antisell's—which, however excusable it might have been in a retained attorney or solicitor of patents, is anything but becoming to the chemical professor or the historian—being content to refer the reader to the published reports of the trial just mentioned. We will here cite only a few lines* descriptive of Selligue's mineral: "The quantity of oily matter in these shales is very variable and often very

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Lighting (Jan. 17th, 1860. vol. ix, p. 41), received as this article is going through the press.

"SETTLEMENT OF THE GREAT TORBANE-HILL CASE.

We have been favored with the following particulars connected with the well known case, the 'Bathgate or Boghead Gas Coal *alias* the Torbane-hill Mineral,' which has lasted upwards of seven years, having passed through several phases in the Supreme Law Courts of Scotland and England. A compromise was finally come to on Wednesday last, the eleventh current. It is embodied in a minute of agreement between Mr. and Mrs. Gillepsie of Torbane-hill, of the first part, and Messrs. James Russel and Son, and James Russel, Esq., of Blackbraes, of the second part.

The preamble of the minute of agreement, which itself consists of twelve articles, is as follows:

'The said parties, considering that disputes and differences have arisen between them and a lengthened litigation has taken place, with respect to the missives of agreement for a lease of certain minerals in the lands of Torbane-hill, entered into betwixt the said first party hereto on the one part, and the said company of James Russel and Son, and individual partners thereof, on the other part, and dated the 30th of March and 1st of April, 1850; and both parties being now desirous that the said litigation should be brought to an end, and all disputes and differences between them amicably adjusted and settled, they have agreed, and hereby mutually agree and bind themselves as follows:'—

The first two articles provide that the actions at present depending shall be abandoned, as a consequence of the execution of the minute.

The third article provides that each party pay their own expenses.

The fourth article, which has for title 'Name of Mineral,' is both an important and curious one—important in a scientific point of view, and curious as illustrative of the pertinacity with which either party have clung to their own views. The article is as follows:—'Whereas the second party have been, and are, working in the said lands of Torbane-hill, and disposing of, under the name of Bathgate or Boghead gas, parrot, or cannel coal, a mineral which they, the second party, deemed and deem to be a parrot or cannel coal, and which the first party deemed and deem to be a new mineral substance, having an argillaceous base, and to be of so peculiar a nature as to constitute it in truth a new and very peculiar variety of bituminous schist, slate, or clay, and have been for some time in use to call 'the Torbane-hill Mineral'; it is hereby agreed that, throughout the remainder of these presents, where the mineral in question is named, it shall be called for the sake of brevity the disputed mineral.'

Article fifth relates to the subject of a portion of Torbane-hill which was reserved from the operation of the mineral lease, and by this article 'the disputed mineral,' as we now call it, contained in the reserved portion of the estate, may be worked or let by the proprietors, without the danger of any obstruction being offered by the second party in the agreement." * * *

* From Dufrénoy et Elie de Beaumont's Explication de la Carte Géologique de la France. Paris, Imp. Royale, 1841. t. i, p. 673.

considerable. According to M. Xardel some rare samples exist which afford even 45 @ 50 per cent; * * * other specimens afford 20 @ 25 per cent. The beds which are worked, or are capable of being worked, yield from 5 to 9 per cent." [p. 675.]—Again [p. 676], "The impressions of fossils, so common in the shales of d'Igornay, occur in the poorer shales. The rich shales, on the contrary, often contain vegetable remains analagous to those commonly found in the coal measures. Perhaps the beds of rich shale are in a manner the representatives of coal-beds; it is to be remarked that in the shale which yields 9 per cent of oil its sheets are covered with a multitude of shining (*miroirtantes*), lenticular veins, having a waxy fracture, which by their aspect and manner of burning recall the variety of coal which is called *cannel coal*." —Leaving it for our readers to answer the question; how far removed in anything but productiveness is the "coal" (Boghead) upon which Mr. Young has operated from the "*shale*" distilled by M. Selligue?

It may not be amiss to mention the fact that upon the continent of Europe the Boghead mineral is almost universally called, not coal, but shale. A fact with which the reader can readily enough familiarize himself by consulting the German chemical journals of the last eight or ten years. In proof of it we cite only the following:* "The recent verdict in the celebrated Torbane-hill-mineral case appears to be contrary to the scientific opinions held in Germany, as proof of which we have a case in point, and which, although not at the time known in this country, was officially decided upon in Berlin previous to the trial coming on in Scotland, which terminated on the 4th of August last. It appears that in Frankfort-on-the-Main there has, for some time past, been in existence a company for lighting the streets and houses by gas from oil, resin, &c. A rival English company contracted to light with coal-gas; and to give both fair play, it was decided that the latter company should be confined to the use of coal alone. Mr. Engelhard, the manager of the Oil (Resin) Gas Company, having heard of the Boghead and Torbane mineral, obtained specimens, and having found they produced excellent gas, gave an order for a large consignment which reached Frankfort *viâ* Rotterdam, through a Dutch agent. This was entered at the Custom-house as cannel coal, much to the annoyance of Mr. Engelhard, who was no more at liberty to make gas from coal than his rivals were to make it of anything else but coal. He was, however, prevented from the necessity of a trial at law, for the officials did not feel themselves justified in charging the duty as coal, although, as other mineral, it would pass free, and applied to higher authorities for instruction. These parties were as much at a loss as their inferiors, and the case was eventually transferred to the Central Board of Customs at Berlin, the last court of appeal of the Prussian *Zollverein*, where all disputed questions in the German States are settled. Scientific men, connected with the board, examined the Boghead and Torbane mineral and decided that it was *not* coal, but *bituminous shale*, which is said to be the general opinion among German chemists. It has been admitted into Germany, duty free, and Frankfort is now partially lighted with gas from this mineral, charged for as resin or oil gas. It is described as a clay containing bitumen, and producing oil when heated. At all events, we may take this German decision as impartial as, had it been admitted as cannel coal it would have been subject to a duty varying from 1*s.* to 1*s.* 6*d.* per ton."

In connection with the question of the products of the distillation of *coal* which Dr. A. would have us believe so entirely new to the world and to this country in particular, we cite the following from The *Encyclopædia of Chemistry*, by James C. Booth and Campbell Morfit. 8vo, Philadelphia, Baird, 1850, p. 461. Article, Coal: paragraph, "products of dry distillation."

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* See *London Journal of Gas Lighting*, Oct., 1853, iii, 256; from *London Mining Journal.*

"These products [of the dry distillation of coal] are somewhat analogous to those derived from wood, and some are identical with them. * * * The liquid products consist of various bodies closely allied to petroleum, and the solids *Naphthaline* and *Paraffine.* The relative proportions of these products vary with the temperature. The lower the heat employed, the less gas, and the more solids and liquids are produced; the higher the temperature the greater is the quantity of carburetted hydrogen." Before closing this sketch we must refer to and correct a palpable error of Reichenbach's* which has been cited by Dr. Antisell (p. 14), from whom we quote it: "So remained paraffine until this hour [date of Y.'s patent], a beautiful item in the collection of chemical preparations; but it has never escaped from the rooms of the scientific man."

Upon the reader who has followed us thus far we need not urge that the above statement is incorrect. As an offset to it we cite the following:†

"In the Parisian Industrial Exhibition for 1839 Selligue exhibited: 1st— Bituminous shale, then fluid bitumen [crude oil], mineral grease, crude and refined (the former at 50 fr. the 100 kilog. [= $5.00 per 100 lbs.]), also *mineral wax* [paraffine] crude and refined (the former at 125 fr., the latter at 180 fr. [= respectively 12½ and 18 dollars per 100 lbs]). The purified mineral wax was beautifully white but the candles made of it had a soiled appearance." V. Hermann (now, according to Wagner, councillor of State in Munich goes) on to assert that "if these fatty products can be prepared economically they belong to the most important objects of the Exhibition."

It would be foreign to our purpose were we to attempt to trace the recent history of the art of manufacturing coal-oil, even if our space allowed of it. During the past few years a large number of papers‡ on the subject have been published in the Scientific Journals of Germany; while several recent works upon the materials used for producing light have each devoted a separate chapter to its description. A few special treatises have also been published of which the following is a, doubtless very incomplete, list.—§

UHLENHUTH, Ed. *Handbuch der Photogen- und Paraffin-Fabrikation aus Torf, Braunkohle und bituminösem Schiefer nach den neuesten Versuchen und Erfahrungen. Quedlinburg, Basse,* 1858.

MUELLER, Carl, Georg, *Die trockene Destillation und die hauptsächlichsten auf ihr beruhenden Industriezweige.* Leipzig, Barth, 1858.

DANCKWORT, MEITZENDORFF *und* WERNECKE. [Committee of the Magdeburg *Gewerbeverein.*] *Ueber das Photogen oder Mineralœl, so wie die ihm*

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* Erdmann's Journal für praktische Chemie, lxiii, 63. Did our space allow, we would gladly transcribe the whole of this article—an English translation of which may be found in the London, Edinburgh and Dublin Philosophical Magazine, [4.] viii, 463—in proof of our assertion that the present widely-spread manufacture of coal-oil and paraffine is mainly due to the comparatively recent discovery of rich stores of highly bituminous substances.

† From v. Hermann's *Die Industrie Ausstellung zu Paris im Jahre,* 1839, *Nürnberg,* 1840. p. 147;—in Wagner's *Jahresbericht ueber die Fortschritte der chemischen Technologie,* 1855, i. 416.

‡ Very complete synopses of these may be found in Wagner's *Jahresbericht,* four volumes of which have thus far been published. For references to the recent admirable *scientific* researches of GREVILLE WILLIAMS, DE LA RUE, and others, which have been chiefly confined however to the more volatile portions of the oil and to the basic compounds which occur in it, see Liebig and Kopp's *Jahresbericht der Chemie, u. s. w.*

§ Small as this list is, it will nevertheless recall to the mind of the reader the modest lines with which Dr. Antisell's preface commences, namely these: "the present little treatise is the first published monograph on the art of distilling oils from minerals containing Bitumen."

ähnlichen Leuchtstoffe, in Bezug auf ihre Feuergefährlichkeit und ihre Anwendung. Magdeburg, 1856.

Also the insignificant brochure of SCHRADER, F. W. *Ueber die industrielle und national-ökonomische Bedeutsamkeit der Gewinnung von Chemikalien insbesondere des Paraffin's und Photogen's aus dem Kohlentheer, u. s. w.* Aschersleben, Beyer, 1856.

This article must here close. Leaving unnoticed several inaccuracies which we had intended to discuss we will dismiss the subject with two brief quotations. The first from Dr. Antisell's book, p. 15. "An impression has taken hold of the American manufacturing public that the patent of James Young has no force, as it was not a new invention at the date of the patent; and from the unfavorable effect of that patent upon the actual manufacture of coal-oils in this country, an ill-feeling has been produced against it. That the owners of this patent have not acted wisely by witholding sales and licenses under it until very lately, is to be regretted; but that it was a *bona fide* improvement in an art at the time when it was patented, and that therefore the patent was rightly issued in this country, there can be no shadow of a doubt in the mind of any one who carefully traces the steps of the discovery of the production of photogenic oils from different materials."

The second from Lord Chief Justice Campbell's charge* to the jury in the case already alluded to. "Now gentlemen I direct you, in point of law, that if there were books then [at date of Young's patent, 1850] in circulation in England disclosing this mode of obtaining paraffine and paraffine oil which were known, were accessible, that the patent would be invalid, although Mr. Young never read those books, and although that mode had not been actually put in practice. If there were books in England in circulation, accessible to all who were interested in the subject, which disclosed this, and would instruct them and enable them to obtain the paraffine and the paraffine oil from the distillation of bituminous substances, then Mr. Young's patent would be invalid."

FRANK H. STORER.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On a probable means of rendering visible the Circulation in the Eye;* by OGDEN N. ROOD, Prof. of Chemistry in Troy University.—Some time ago while looking at a bright sky through three plates of cobalt-glass, I saw with astonishment that the field of view was filled with, and traversed in all directions by small bodies resembling animalcules.

They were seen on the blue field as yellowish spots, and always appeared elongated in the direction of their motion, which was as a general thing tolerably uniform. The same result was obtained by experimenting upon the eyes of a number of persons.

Convex lenses of various foci, (from 3 in. to ½ in.), were now held before the eyes, so as to give the blue light various degrees of convergence and divergence, without in the least altering the appearance of the moving bodies; this seemed to indicate that their locality was in the retina or in its immediate neighborhood.

A position near the axis of vision was selected, and observed, when it was found that these bodies in traversing this spot always pursued the same direction and path, disappearing at the same point; other positions near the axis gave like results.

* *Loc. cit.*, p. 520.

This would seem to preclude the possibility of the moving bodies being animalcules swimming in the humor of the eye; the most probable remaining supposition is, that they are blood-corpuscles circulating in the retina or in its immediate neighborhood. The apparent diameter of these bodies when seen projected on a window six feet distant may be about $\frac{1}{20}$ of an inch, which corresponds to about $\frac{1}{1600}$ of an inch on the retina. The average diameter of the blood globules is $\frac{1}{3200}$ of an inch, but taking into account the fact that the shadows of the moving bodies are not well defined the correspondence may be considered pretty satisfactory.

The question now arises as to the manner in which the blue glass renders the circulation visible, for these moving shadows cannot be seen with distinctness through red, orange, yellow, green, nor even purple media; they are on the other hand well shown by a certain thickness of a solution of the cupro-sulphate of ammonia.* Yellow solutions when combined with the blue glass or blue solutions render the circulation invisible, and it does not reappear till the yellow solution has been made so dilute as barely to preserve a yellow tint, and to transmit the spectrum almost unaltered. This shows that the indigo and violet rays are principally concerned in the production of this appearance, but that it cannot be attributed to fluorescent properties in the blood discs is indicated by the fact that the circulation can be seen through a considerable thickness of crown glass, through an infusion of red sanders wood mixed with ammonia, as well as through a solution of the disulphate of quinine.

The only explanation that has occurred to me as being probable is the following: the blood discs are yellow and consequently opaque to a great extent to the indigo and violet rays; they would therefore in passing before the retina cast shadows on it; now the retina being already strongly impressed with blue light, that portion of it which was momentarily protected from the action of this light, would experience the complimentary sensation—or would see instead of a moving shadow a yellowish moving streak. This explains also why the appearance is not seen with any distinctness in red, orange, yellow, or green light, for yellow media are to a great extent transparent to all their rays and therefore fail to cast shadows. These observations if new may be of some interest to those engaged in the study of the physiology of the eye.

Troy, May 14th, 1860.

Technical Chemistry.

1. *Care of Platinum Crucibles.*—In connection with some sensible remarks upon the use of sand in cleaning platinum crucibles,—a practice which, with Berzelius, (Lehrbuch der Chemie, 1841, 4th Aufl. p. 516,) he heartily commends—urging that it should be employed every time that a crucible is used, Erdmann explains the cause of the gray coating which forms upon platinum crucibles whenever they are ignited in the flame of Bunsen's gas-burner.

This coating has given rise to much annoyance and solicitude among chemists. Indeed it has often been asserted, that the use of Bunsen's

* Mr. Wm. B. Taylor of Washington to whom some of these facts were communicated by Prof. J. Henry was able to trace this appearance, though with *diminishing distinctness*, through plates of bluish-green and yellow glass.

burner is unadvisable in quantitative analysis, since by means of it the weight of platinum crucibles is altered and the crucibles themselves injured. The coating is produced most rapidly when the crucible is placed in the inner cone of the flame, and the more readily in proportion as the pressure under which the gas is burned, is higher. Having found it advantageous to maintain, by means of a special small gas holder, a pressure of four or five inches upon the gas used in his own laboratory, Erdmann has observed that the strong gas flame thus afforded, immediately occasions the formation of a dull ring upon the polished metal placed in the inner flame, this ring being especially conspicuous when the crucible becomes red hot; it increases continually so that after long continued ignition the whole of the bottom of the crucible will be found to be gray, and with its lustre dimmed.

This ring is caused neither by sulphur, as some have believed, nor by a coating of inorganic matter, but is simply a superficial loosening of the texture of the platinum in consequence of the strong heat; whence it first of all appears in the hottest part of the flame.

In conjunction with Pettenkofer, Erdmann instituted several experiments which have left but little doubt that the phenomenon depends upon a molecular alteration of the surface of the metal. If a weighed polished crucible be ignited for a long time over Bunsen's lamp, the position of the crucible being changed from time to time in order that the greatest possible portion of its surface shall be covered with the gray coating, and its weight be then determined anew, it will be found that this has not increased. The coating cannot be removed either by melting with bisulphate of potash or with carbonate of soda. It disappears, however, when the metal is polished with sand; the loss of weight which the crucible undergoes, being exceedingly insignificant, a crucible weighing 25 grams, having lost hardly half a milligram. When the gray coating of the crucible is examined under the microscope it may be clearly seen that the metal has acquired a rough, almost warty, surface, which disappears, when it is polished with sand. Platinum wires which are frequently ignited in the gas flame, for example the triangles which are used to support crucibles, become as is known, gray and brittle. Under the microscope they exhibit a multitude of fine longitudinal cracks which as the original superficial alteration penetrates deeper become more open, or as it were spongy—until finally the wire breaks.

If such wire is strongly and perseveringly rubbed with sand, the cracks disappear, and the wire becomes smooth and polished, for the grains of sand acting like burnishers restore the original tenacity of the metal; very little of its substance being rubbed off meanwhile. The loosening effect of a strong heat upon metals is beautifully exhibited when silver is ignited in the gas flame; a thick polished sheet of silver immediately becoming dull white when thus heated. Under the microscope, the metal appears swollen and warty. Where it has been exposed to the action of the inner flame along its circumference, this warty condition is visible to the naked eye. A stroke with the burnishing stone however, presses down the loosened particles, and reproduces the original polish. This peculiar condition which the surface of silver assumes when it is ignited, is well-known to silversmiths, it cannot be replaced by any etching with

acids; and it must be remembered that what is dull white in silver, appears gray in platinum.

If each commencement of this loosening is again destroyed, the crucibles will be preserved unaltered, otherwise they must gradually become brittle. Crucibles of the alloy of platinum and iridium are altered like those of platinum when they are ignited; it is, however, somewhat more difficult to reproduce the original polish of the metal, by means of sand, as might be expected from the greater hardness of the alloy.

The sand used should be well worn; when examined under the microscope, no grain of it should exhibit sharp edges or corners, all the angles should be obtuse.—*Journ. für praktische Chemie*, March, 1860, lxxix, 117.

2. *Safe and easy method of rectifying Sulphuric Acid;* by N. NEESE of Kiev.—"The rectification of sulphuric acid offers an example of how readily even the most practical matters may fall into oblivion in spite of the copiousness and the vigilance of our literature. Fifteen years ago, Prof. Siller, at that time filling the chair of Pharmacy at Dorpat, called my attention to an exceedingly ingenious device for rectifying sulphuric acid, which he had read in some journal. The retort in which the rectification of the acid is to be conducted, should be placed upon a bed of ashes about an inch in thickness, and sand then heaped up all around it; the distillation may then be proceeded with, without any further precautions. By this method I have twice rectified sulphuric acid, operating with a very bad retort, upon portions of 15 lbs weight; and have been astonished at the facility and tranquillity with which the distillation proceeded. The ashes evidently act as a non-conductor, so that the acid can boil only from the sides of the retort. Since then, manifold expedients have been proposed, all depending upon some peculiar apparatus, constructed for this particular purpose, but I have not yet met with any reference to the simple contrivance which has here been mentioned."*—From *Archiv der Pharmacie*, cxlv, 267; in *polytechnisches Notizblatt*, 1860, xv, 43.

[FREDERKING of Riga, (*Archiv d. Pharm.*, Aug., 1859; in *American Journal of Pharmacy*, viii, 88,) corroborating the statement of Neese, states that he can recommend the process from his own experience,

* The author is evidently not aware that several devices almost as simple as the one which he describes have previously been proposed. Thus Otto, (in his *Lehrbuch der Inorganischen Chemie*, Braunschweig, 1855, i, 275,) directs that the retort be caused to rest upon an inverted crucible cover, which is placed upon a grate in an extemporaneous fireplace constructed of loose bricks, (figured in Otto,) so that the bottom of the retort may be less strongly heated than the upper portions by the charcoal fire which is built around it. Otto, however, considers it necessary to cover the retort with a thin coating of clay which has been mixed with a dilute solution of carbonate of soda or of borax, instead of water, so that it may adhere firmly to the retort when heated; urging that the danger of fracturing the latter is not only lessened thereby, but that a more rapid distillation is insured, since little or no condensation can occur within a retort thus protected. In Mohr, Redwood and Procter's *Practical Pharmacy*, Phila., 1849, p. 337, a somewhat similar proposal is made A Hessian crucible being employed, however, instead of the cover used by Otto:—moreover, the retort is not coated.

Possibly a more convenient method than either of the above would be to fill a sufficiently capacious crucible with ashes, or some other non-conducting substance—calcined gypsum, for instance,—as a bed upon which to rest the bottom of the retort. A charcoal fire similar to Otto's being then built around it. F. H. S.

inasmuch as he has during the last twelve years, repeatedly rectified sulphuric acid in the manner indicated.—F. H. S.]

3. *Vulcanization of Caoutchouc, by means of mixed Sulphur and Hypochlorite of Lime.*—GAULTIER DE CLAUBRY, having detected the presence of chlorid of calcium in many samples of vulcanized rubber, and suspecting that this salt might have been derived from hypochlorite of lime employed in some modification of Parkes' process of cold sulphuring, [English Patent of March 6, 1846. Repertory of Patent Inventions, (E. S.,) ix, 46,] to produce chlorid of sulphur in the rubber paste, was led to perform the following experiments:

When flowers of sulphur and dry hypochlorite of lime, (bleaching powder,) are shaken together, a very strong odor of chlorid of sulphur is immediately developed. If the mixture be somewhat forcibly rubbed in a mortar, elevation of temperature ensues, the sulphur softens and the mixture becomes solid while abundant vapors are evolved. When a much larger amount of sulphur than of the hypochlorite is used and friction is avoided when the two are blended, a mixture is obtained which being added to the caoutchouc paste—either with or without the addition of inert matters, such as chalk, oxyd of zinc, etc., serving to give "body" to the product—effects the vulcanization of the latter either at the ordinary temperature, or when gently heated. By this means, objects of any thickness can be uniformly vulcanized.

If instead of employing an excess of sulphur, an excess of the hypochlorite be introduced into the mixture, and this be agitated, so much heat will be developed that the vessel containing the mixture can no longer be held in the hands; if the flask be closed, the action becomes so violent that the cork will be blown out, or the flask broken by a violent explosion.—*Comptes Rendus*, May, 1860, l, 876.

4. *Preparation of Cyanid of Barium, and of Ammonia with the Nitrogen of the Air;* by MARGUERITTE and DE SOURDEVAL.—In a brief preliminary note, the authors claim :—

That baryta when calcined in the presence of charcoal and of atmospheric air combines very readily with carbon and nitrogen, cyanid of barium being formed.

That the cyanid of barium heated in a current of aqueous vapor is decomposed at a temperature of about 300° (C.) [572° F.] and disengages in the form of ammonia, all the nitrogen which it contains.

Trials which they have made upon a tolerably large scale, have been eminently successful, leading them to hope that not only the various cyanids employed in the arts, but also ammonia and nitric acid may thus be economically produced.*—*Comptes Rendus*, June, 1860, l, 1100.

5. *Gun-Cotton Filters;* by Prof. BOETTGER.—Since gun-cotton—itself a product of the action of strong acids—when properly prepared is scarcely at all acted upon at the ordinary temperature, by chemicals, being capa-

* To prepare baryta from its carbonate, M. and De S. ignite a mixture of the latter with the pitch ("asphaltum") of coal tar. Each molecule of the carbonate being thus brought in contact with the reducing agent, carbon, excellent results are obtained, the decomposition of the carbonate being easy and the product of baryta abundant. From observing the odor of ammonia which was at times developed during their experiments upon this method of preparing baryta the authors were led to the important discoveries which are noticed in the text.—*Répertoire de Chimie appliq.* June, 1860, ii, pp. 169, 170.

ble of withstanding the most corrosive agents; it affords a material for filtering strong acids, and the like, as well as liquids which would be decomposed by contact with the organic matter of ordinary filters, the excellence of which cannot be too highly extolled.

Besides employing it for removing from strong nitric acid the chlorid of silver which is precipitated in the common method of purifying this acid by means of nitrate of silver, (as has recently been advised in the Berlin *polytechnisches Intelligenz-Blatt*, No. 4, p. 30); Boettger affirms that, he has for several years past, found it of special use in filtering off the slimy precipitate, containing selenium, which is gradually deposited, when fuming sulphuric acid is mixed with a little water; in separating crystallized chromic acid from the sulphuric acid of the mother liquor; and in filtering concentrated solutions of permanganate of potash, in order to separate suspended peroxyd of manganese. He has even found the gun-cotton well suited for filtering concentrated alkaline lyes, aqua-regia, and concentrated solutions of chlorid of zinc, not to allude to many other instances. In using the cotton a small bit of it is pushed loosely, like a stopper, into the throat of a funnel.

The materials which have heretofore been used for similar purposes, viz.: garnets, asbestos, powdered glass, &c., are very much inferior, as filters, to the loose, fibrous gun-cotton.—From *polytechnisches Notizblatt*, 1860, No. 7; in Dingler's *polyt. Journal*, clv, 463.

6. *Preservation of Flesh;* by VERDEIL.—Having been separated from the bones, and, as far as possible, from fat, the flesh is cut into slices from one to five centimetres, (one centimetre =0·3937 inch,) in thickness; the slices being cut as nearly as possible across the grain of the flesh. These are now laid upon hurdles of basket-work, which are subsequently placed in a chamber. As soon as a sufficient number of the trays have been introduced into the chamber, it is closed, and steam under a pressure of three or four atmospheres, consequently of 135° to 145° C. [=275° to 293° F.] is admitted through several openings.

The chamber, which may be of lead or iron, must not be absolutely tight, a small outlet for the steam being necessary, in order that the pressure may not become too great.

After from six to ten or fifteen minutes, according to the kind of flesh and the thickness of the slices, the steam is shut off, this part of the process being finished.

The flesh is now very nearly in the condition of boiled meat, but has retained all of its ingredients, the albumen having been coagulated: its taste recalling that of roasted meat. It presents a wrinkled appearance, is of a gray color, and may be readily divided.

Being removed from the steam chamber the flesh is now placed upon trays, or hung upon hooks, in another chamber which is warmed, but in which the temperature is never allowed to exceed 40° or 50° C. [=104° to 122 F.]. The drying process is completed in the course of eight or twelve hours.

Packed in tight casks or in tin boxes, so that it may be protected from the action of moisture, and from insects, the flesh thus prepared may be preserved for any length of time which may be desirable. It is nevertheless well to place a layer of salt in the casks, in order that it shall absorb any moisture which the flesh may have retained. Before using

this meat it must be soaked for an hour or two in warm water in which it softens and regains its original condition. When boiled with water it affords an excellent soup, and passes into a condition, in which it cannot be distinguished from fresh meat.—From *Le Génie Industriel;* in BOETTGER's *polytechnisches Notizblatt*, 1860, xv, 71.

7. *Magnesium as a Source of Light.*—Prof. A. SCHMITT calls attention to the practicability of employing metallic magnesium for purposes of illumination, as had already been suggested by Bunsen. From the researches of the last named chemist, it is known that when magnesium is ignited it readily takes fire and burns with an exceedingly brilliant flame. The intensity of the light thus produced, as determined by Bunsen and Roscoe in one of their photo-chemical researches (*Pogg. Annalen*, cviii, 261, *et seq.*) is only some 525 times less than that of the sun. Compared with an ordinary candle, it appeared that a wire of magnesium 0·297 millimetre [1 mm. =0·0394 inch] in diameter, produced as much light in burning as 74 stearine candles, five to the pound. In order to support this light during one minute, a piece of wire 0·987 metres long, weighing 0·1204 gram [1 grm. =15·4325 grains], was required.

Only 72·2 grams of magnesium, therefore, would be needed, in order to maintain during ten hours an amount of light equal to that of 74 stearine candles, consuming about 10·000 grams of stearine.

According to Bunsen, magnesium wire is readily obtained, by forcibly pressing the metal through a hot steel die by means of a steel piston. Bunsen's arrangement for burning the wire was made, by connecting spools of it with rollers moved by clock-work so that the wire should be unrolled like the ribbon of paper in Morse's telegraph, the end of the wire thus gradually pushed forward, passed into the flame of an ordinary alcohol lamp, where it took fire.

It is evident that a magnesium lamp of this sort must be much simpler and more compendious than any of the existing arrangements of the electrical, or of Drummond's light; for light-houses, &c.: where an intensely brilliant illumination is required it can hardly fail to rival either of these. Where an extraordinary amount of light is needed, it could readily be produced by burning large wires, or several thin ones at the same time. Another important consideration is the fact that the spools of wire, as well as the clock-work and spirit lamp, are easily transportable.

It is not, however, to the intensity alone of the magnesium flame that these lamps owe their utility, for the photochemical, (*i. e.*, photographical) effect of the light is also very great. According to Bunsen, the photochemical power of the sun being only 36·6 times greater than that of the magnesium flame. The latter must therefore be useful in photographing by night or in any dark or subterranean locality; the evenness and remarkable tranquillity of the flame, especially commending it for this purpose.

The present high price of magnesium, it is true, must prevent any extended use of it for technical purposes. For example, Lenoir of Vienna charges 3 Florins, [1 Fl. =51 cts.] for a gram of it, hence the cost per minute of the light just described, would be 36 *Neukreutzer*, [1 ktr.= about $\frac{5}{8}$ of a ct.,] and the cost during ten hours, would amount to 216 Florins, while the ten kilogrammes of stearine could be procured for less than 14 Florins. But even at this price, it could still be used by photo-

graphers, since it would only be required for exceedingly short intervals of time, and all unnecessary consumption of the wire might be prevented by stopping the clock-work.—From *Stamm's Illustr. Zeitschrift*, 1859, p. 332; in *polytechnisches Notizblatt*, 1860, xv, 56.

8. *Method of employing carbonic acid in connection with the hypochlorite of lime used for bleaching paper-stock.*—An apparatus devised by FIRMIN DIDOT and BARRUEL of Paris, for introducing carbonic acid, prepared by burning charcoal, into the solution of hypochlorite of lime, (bleaching salt,) while the latter is in contact with the fibre which is to be bleached, is described in the Nov., (1859,) No. of Barreswil's *Répertoire de Chimie*, Appliquée, vol. i, p. 457.

The carbonic acid on being introduced into the solution of bleaching salt, unites with the lime, thus setting free hypochlorous acid, the decolorizing action of which is infinitely more energetic when it is at liberty than when in combination with a base.

This process, says Barreswil, is of extreme simplicity, and one is at a loss to comprehend why it had not been sooner invented, in view of the fact that each and all of its phases have been so long and so well known.

In order to judge of the practicability of the new process—in so far as concerns difference of price, strength, and whiteness of the paper, and the duration of the operations in the two systems, (new and old,) of bleaching—comparative experiments were instituted, by the Messrs. Firmin Didot, upon carefully assorted rags. The cost of the chemicals and labor, and the amount of time required, having been exactly noted. After bleaching, the pulp was converted into paper. The different papers were then carefully tested. As the result of these experiments, it appeared that the new process was more energetic and more rapid than the old method, *au chlore*, [*chlorure?*] *liquide*, [with solution of bleaching salt,] and that in many cases it is also equally energetic with the process in which chlorine gas is employed. Over the latter it has the advantage of not destroying to so great an extent, the fibre of the pulp.

Since the details of the process, which for that matter consists merely of arrangements for thoroughly washing and cleansing the carbonic acid employed—the latter being then introduced into the bleaching vats, just as if it were steam, through coils of pipe pierced with holes, which are placed at the bottom of the vats,—cannot well be explained without a diagram, we must refer the reader who may desire these to the original article, in which the apparatus is figured. F. H. S.

9. *New "fusible metal."*—Dr. B. WOOD of Nashville, Tenn., has secured a patent (Weekly Scientific Artizan, Cincinnati, May 5th, 1860,) for an alloy composed of cadmium, tin, lead and bismuth, which fuses at a temperature between 150° and 160° F. The constituents of this *fusible* metal may be varied according to the other desired qualities of the alloy—viz: cadmium one to two parts; bismuth seven to eight parts; tin two parts; lead four parts. It is recommended as being especially adapted for all light castings requiring a more fusible material than Rose's or Newton's "fusible metal," it having the advantage of fusing at more than 40° F. lower temperature than these alloys, and owing to this property may replace many castings heretofore made only with amalgams. Its fusing point may be lowered to any extent by the addition of mercury, which

may be employed within certain limits without materially impairing the tenacity of the metal. In a letter to the Editors, dated Nashville, June 9th, 1860, Dr. Wood says:—

"One point in particular that strikes me as being worthy of note is the remarkable degree in which Cadmium possesses the property of promoting fusibility in these combinations. The alloy of one to two parts *cadmium*, two parts lead, and four parts tin is considerably more fusible than an alloy of one or two parts *bismuth*, two parts lead and four parts tin; and when the lead and tin are in larger proportion the effect is still more marked. It takes *less* cadmium to reduce the melting point a certain number of degrees than it requires of bismuth, besides that the former does not impair the tenacity and malleability of the alloy, but increases its hardness and general strength.

Bismuth has always held a pre-eminent rank among metals as a fluidifying agent in alloys. Its remarkable property of 'promoting fusibility' is specially noted in all our works on chemistry. But I do not find it intimated in any that cadmium ever manifests a similar property. The fact indeed appears to have been wholly overlooked—owing perhaps to the circumstance that as an alloy with certain metals cadmium does not promote fusibility.

Cadmium promotes the fusibility of some metals, as copper, tin, lead, bismuth, while it does not promote the fusibility of others, as silver, antimony, mercury, &c., (i. e., does not lower the melting point beyond the mean.) Its alloy with lead and tin in any proportion and with silver and mercury, within a certain limit, say equal parts and especially of two parts silver and one of cadmium or two parts cadmium and one mercury are used, are tenaceous and malleable, while its alloys with some *malleable* metals, (gold, copper, platinum, &c.,) and probably with all brittle metals *are* 'brittle.'

I notice a great discrepancy among authors as to the melting point of this metal. It is usually put down the same as that of tin, (442° F.) Brande (*Dict. of Science and Arts,*) says it 'fuses and volatalizes at a temperature a little below that at which tin melts.' Daniell, (according to the *New American Cyclopedia,*) gives its melting point at 360° F., while Overman places it at 550° and gives 600° as the temperature at which it volatalizes.

The latter is doubtless the nearest the truth. The metal requires for its fusion a temperature too high for measurement by the mercurial thermometer, but from relative tests with other metals I should place its melting point in round numbers at 600° F. as it melts and congeals nearly synchronously with lead, the melting point of which is stated by different authorities as 594°, 600°, and 612° F. It volatalizes at a somewhat higher heat.

I draw attention to these facts believing that the metal possesses properties valuable to Art and interesting to Science, and that it merits more thorough investigation than appears to have been bestowed upon it."

[We have had time only to repeat a few of Dr. Wood's interesting experiments in regard to the remarkable influence which cadmium exercises in lowering the fusible point of various alloys. The alloy made by fusing together two parts of cadmium, two parts tin, four parts lead and eight parts bismuth melts at a temperature varying not far from 70° C. (158° F.) It may appropriately be called "*Wood's fusible metal.*"—EDS.]

II. GEOLOGY.

1. *Note from* Dr. Newberry, *in reply to* Mr. Lesquereux, (in a letter to the Editors).—I see by the note from Mr. Lesquereux, [contained in this Journal, xxix, 435,] that my letter from Santa Fé was unacceptable to him. This both surprises and grieves me, as the thought that he might be drawn into the controversy had not occurred to me; and I am sincerely sorry to learn that one with whom I have had so many years of friendly intercourse could so readily misconstrue both the statements and the spirit of my letter. Possibly its tone may have failed to reflect the great respect which I have had and still have for Prof. Heer; and to others than Mr. Lesquereux it may have seemed not altogether courteous. It should be borne in mind, however, that the discussion in reference to these fossil plants, and the age of the strata containing them, had already been repeatedly brought before the public; and that in this discussion the tone of the associate of Prof. Heer had been marked by a degree of arrogance difficult to bear patiently. Prof. Heer had called them *Miocene*—an error which with the imperfect material in his hands was natural enough; and one which should detract nothing from his high reputation—but by the testimony of several observers they had been proved *Cretaceous*. Ignoring their testimony, however, and adhering to his former opinion, a portion of his letter to Mr. Lesquereux was written to perpetuate what I knew to be a mistake. It also did me, as I conceive, manifest injustice. That letter reached me when I had been for months in exile, and where I was surrounded by proofs of the truth of the position I had before taken—circumstances favorable to the development of a little honest indignation. In the freshness of that feeling my reply was written, and I am willing to admit, if others think so, that it was not sufficiently respectful.

So much for the manner of my letter. In regard to its statements of fact I fear I shall be unable to make any such concession. On the contrary, my regard for truth requires that I should repeat each and all of them.

(1.) Prof. H. considered the plants in question Miocene. There is not the shadow of a doubt that they are Lower Cretaceous.

(2.) Prof. H. states that "*except Credneria* and *Ettingshausenia all the genera enumerated* (in my list) *are represented in the Tertiary and not in the Cretaceous.*" It will be observed that he does not say they are *characteristic* of the Tertiary, or "of the Tertiary," as Mr. Lesquereux quotes him—but distinctly affirms that they *are not represented in the Cretaceous.*" Hence there is no propriety in the remarks of Mr. Lesquereux on this point; and the error in the statement of Prof. Heer shown by reference to Stichler's paper, before quoted, remains unexplained. If that error was not accidental, it was designed. If accidental, as I cannot for a moment doubt, the offensive clause of my letter is no more than just. If designed, stronger language would be admissible. The appeal to "authority" has been nearly exhausted in this discussion, and the time has passed when personal influence could make errors pass for truths. Prof. Heer is a man of estimable character, of

great learning and of world-wide reputation, and, I am sure, would be one of the last to ask us to believe a scientific statement simply because he had made it.

(3.) In my letter I made no supposition in reference to the Tertiary flora of Kentucky, Tennessee or Mississippi. I merely stated some facts in reference to the Miocence flora of the country bordering the Upper Missouri, 1000 miles distant from the nearest of these States. I also distinctly said that the absence of tropical plants from the collections made there, was only negative evidence. They may be found in that region to-morrow, but at the time of writing that letter they had not been found, and all the material in my hands indicates, as I then said, a Tertiary climate warmer than the present, but still temperate.

(4.) I was also fully aware that marine Tertiary deposits extend up the Mississippi even higher than stated by Mr. Lesquereux. I excluded them from "*the central portion of the continent;*" by this meaning, as I then explained, the region between the Mississippi and the Sierra Nevada. Here, too, the evidence is negative, but now stands just as I represented it.

(5.) Mr. Lesquereux says: "I cannot admit, as Dr. Newberry appears to do, that the fossil flora of the American Cretaceous, ought to be closely related to the European." My only reference to this question will be found on page 216 (Journal, March, 1860), where I say—"We may find hereafter, in other parts of the continent than those in which I have examined the Cretaceous strata, fossils which shall assimilate our flora of that period more closely to that of Europe, but, so far as at present known, our plants of this age present an *ensemble* quite different."

(6.) The statement made by Mr. Lesquereux that "the age of the strata from which American fossil plants have been taken is mostly uncertain," is manifestly incorrect. At least nine-tenths of the species enumerated are from the Carboniferous and Devonian rocks, whose place in the geological series is certainly well ascertained. Of those collected and not yet catalogued, perhaps an equal proportion have been obtained from the Cretaceous and Miocene strata, of which the places in the series have been as accurately determined, by the molluscous fossils which they contain.

(7.) It is true that in America fossil botany has had but few devotees, and doubtless all of them have at times keenly felt the want of more books and specimens bearing on their subjects of study. Still, I believe everything that has been published in reference to fossil plants is accessible to the American student within the limits of his own country. At the same time it is also true that a satisfactory comparison between the extinct floræ of Europe and America can only be made by means of full collections of well-marked *specimens*, many more than we yet possess.

Mr. Lesquereux is aware, as is every one who has given the subject any attention, that our knowledge of the floræ of the different geological formations has been limited, not so much by the want of learning and acuteness in the cultivators of fossil botany, as by the small number and imperfect preservation of the fossil plants collected. It could hardly

be otherwise, then, than that in the whole New World material should be discovered which should throw new light on the ancient vegetation of the globe. The idea that no American can be qualified to make good use of such material, is another instance of the arrogance to which I have before alluded, and to which it would be unmanly tamely to submit.

III. BOTANY AND ZOOLOGY.

1. *Geological and Natural History Survey of North Carolina. Part III. Botany; containing a Catalogue of the Plants of the State, with Descriptions and History of the Trees, Shrubs, and Vines.* By Rev. M. A. CURTIS, D.D. Raleigh, 1860: the First part only, the *Woody Plants of North Carolina.* pp. 123, 8vo.—We have turned over the pages of this popular exposition with much interest, and gleaned some valuable information. "Botanists will of course find fault with it," says the author, who we well know could write scientifically and profoundly enough, if he so pleased, but who has here come down to the level of his most unlearned readers, discoursed separately of trees, shrubs, and vines, and classified these in a fashion which might well shock the susceptibilities of a stickler for technical nomenclature and natural system in botany. Now, we are not shocked at all; indeed we quite enjoy a glimpse of Flora *en deshabille* and slip-shod, and are well aware how much easier it is, and how much better in such cases, to fit your book to its proper readers than to fit the readers to it. The fault we should find is not with the plan of this Report, but with the quantity. We could wish for more of it, for a volume as large at least as Mr. Emerson's Report on the Trees and Shrubs of Massachusetts. We quite like to see the popular names put foremost, but would suggest that the botanist who does this should *lead* as well as follow the indigenous nomenclature, so far as to correct absurd or incongruous local names, and introduce right or fitting ones as far as practicable. For instance *Virgin's Bower* is not a proper name for *Wistaria frutescens*, and is rightly applied to *Clematis Virginiana* over the leaf. (We venture to add, in passing, *C. Viorna* to the list, having gathered it in Ashe County.) And, although the people along shore call *Baccharis* by the name of the English annual weed, *Groundsel*, it were better to write it *Groundsel-tree*. *Yellow wood* is the name of *Cladrastis*, rather than of *Symplocos*, which the Carolinians call *Horse-Sugar*. Dr. Curtis can coin a name upon occasion; for surely nobody in Carolina knows *Menziesia globularis* as *False Heath*, nor has it any scientific claim to this appellation. While in critical mood we may express a strong dissent from the proposition that *Rhododendron punctatum* is too inferior to the other two species "to attract or deserve much attention." With us, it is surpassingly beautiful in cultivation, none the less so because its habit is so different, having light and pendent branches, when well grown forming broad and thick masses, and loaded with its handsome rose-colored blossoms. While *Leucothoë Catesbæi* is called "a very pretty shrub," the far handsomer *Andromeda floribunda*, so much prized by our nurserymen, gets no commendation. *Magnolia Fraseri* may not only be "cultivated in the open air near Philadelphia," but is perfectly hardy near Boston, and the earliest to blossom; but we never noticed the fragrance of the flow-

ers. On the other hand, as it is native as far south as Florida, it might thrive in plantations any where in North Carolina. The flowers of *M. cordata* are described as if larger than those of *M. Fraseri*, instead of the contrary; we could hardly say much for their beauty, except in comparison with those of the common Cucumber-tree. *Prunus Virginiana* is omitted; yet surely it is not wanting in North Carolina. And it is almost an excess of conscientiousness to leave out *Cladrastis*, the handsomest tree of the country, all things considered, when it is known to grow only a few rods over the Tennessee line.

On the other hand, we are disposed to doubt if the genuine *White Spruce*, (*Abies alba*,) occurs in North Carolina. At length we know this tree, but only in Canada and parts adjacent. It is more, instead of less northern in its range than *A. nigra*. But since President Wheeler has pretty nearly determined the existence of *A. Fraseri* on the Green Mountains in Vermont, we could not deny that *A. alba* grows with the latter on the high mountains of North Carolina. We make our little criticisms freely,—as we know the excellent author would wish,—for we think it likely that this part of the Report will pass to a second edition,—when we hope it will be largely augmented. A. G.

2. *Thwaites*, *Enumeratio Plantarum Zeylanicæ*, Parts I. II. 8vo, pp. 160. 1858–1859.—A complete enumeration of the known plants of Ceylon, with characters of new or little known genera and species, and numerous descriptions and critical remarks, the synonymy, &c., elaborated by Dr. Hooker. These two published parts extend from *Ranunculaceæ* to *Compositæ;* so that a good-sized volume will complete the work, and constitute an important adjunct to the great Indian Flora. A. G.

3. *Walpers*, *Annales Botanices Systematicæ*, continued by Dr. C. Müller, Berol.—Five parts of the fifth volume are published, extending to page 800, and to the order *Coniferæ*. A. G.

4. *Bueck*, *Index ad De Cand. Prodromum, etc.* Pars III. Hamburg, 1859. pp. 506.—This useful Index to De Candolle's Prodromus is here continued from the second part of the seventh to the thirteenth volume. As we may expect that at no distant period the Prodromus will be terminated, as announced, we trust that the next Index will combine the whole into one continuous alphabetical list. A. G.

5. *Synopsis Methodica Lichenum omnium hucusque cognitorum, præmissa introductione lingua Gallica tractata, scripsit* William Nylander. Fasciculus II, Parisiis ex typis L. Martinet via dicta Mignon, 2, 1860. 8vo. pp. 141–430.—We are glad to welcome another portion of the important work of Dr. Nylander, which is indispensable to every Botanical library. Beginning with the *Caliciei*, the present part embraces the *Bæomycei*, the *Cladonici*, the *Usneei*, and the *Parmeliei*, ending with the genus *Physcia*. The higher tribes of Lichens are by no means the least difficult, and nothing in the part before us is more acceptable than the author's elaboration of the genus *Sticta*;—disposed by him in *Sticta*, *Stictina*, and *Ricasolia*. The last general synopsis of Lichens, that of Acharius, was published nearly fifty years ago, and the vast amount of valuable matter, scattered in many publications, which has since been accumulating, has long needed to be brought together in one work. This Dr. Nylander proposes to accomplish, adding also the results of his investigation of all the

most important collections; and disposing the whole in a system, which, while it aims to retain all that was most valuable in the old, gives us, as it should, the whole light of modern (microscopical) science upon both old and new. We have only room to add here that "*Usnea lacunosa, Willd.*," is a name found only in Willdenow's herbarium, and was anticipated in print by *U. cavernosa*, published by the present writer, in the appendix to Agassiz's tour to Lake Superior. Messrs. Westermann and Company of New York, will receive subscriptions to the synopsis, which is put at a moderate price for so handsome a book. E. T.

6. *Reports of Explorations and Surveys* to ascertain the most practicable and economical route for a Railroad from the Mississippi River to the Pacific Ocean, made under the direction of the Secretary of War, in 1853–6, &c., vol. x. Washington, 1859.—FISHES; by CHARLES GIRARD, M.D. Washington, D. C., 1858.

Of recent contributions to our knowledge of special Faunas, none have been of greater importance or interest than the report on the Fishes of Western North America. In this volume, Dr. Girard has incorporated almost everything known to the date of publication concerning the Ichthyology of our Pacific possessions.

In the introductory remarks, (which with some variations, are duplicated) a general view is given of the Piscine Fauna of Western North America. The families which are richest in genera and species, and which are most characteristic of the Californian Fauna especially, are those of the Cataphracti, the Blennoids, the Embiotocoids. and the Pleuronectoids. Of all these families, many new genera, previously indicated in the "Proceedings of the Academy of Natural Science of Philadelphia," are described and illustrated.

The classification of the late Johannes Muller is adopted, and the orders, suborders and families of which representatives are described, are all characterized; the genus *Amblodon*, however, is retained in the family of Sciænoids; this should in strict accordance with the principles of that classification, be transferred to the order of Pharyngognathi. Agassiz has demonstrated the union of the lower pharyngeal bones, the only character on which the order depends. Dr. Girard does not appear to have noticed this discovery as he has not adverted to it in the generic diagnosis of *Amblodon*.

Of the family of Percoids, representatives of only one genus are yet known as inhabitants of the Pacific coast. This genus has been described as new under the name of *Paralabrax*, and includes two species, both of which had been first referred to *Labrax* in the Proceedings of the Academy. It is here placed in the vicinity of *Serranus*.

Many fresh water Percoids are described, belonging to the genera *Dioplites* Raf., *Pomoxis* Raf., *Ambloplites* Raf., *Calliurus* Raf., *Bryttus* Val., *Pomotis* Raf., *Labrax* Cuv., and *Stizostedion* Raf. The species described have been collected in many distant places west of the Mississippi river, but one species (*Ambloplites interruptus*) having been obtained in California.

The name of *Dioplites* has been substituted for *Grystes* of Cuvier. It would by many have been deemed more proper to have applied Rafinesque's generic name of *Lepomis*. Rafinesque in his "Ichthyologia Ohi-

ensis" has characterized the genus *Lepomis* and divided it into two subgenera, *Aplites* and *Dioplites*, giving at the same time, to each of the species, the generic name of *Lepomis*—*Aplites* and *Dioplites* having been separated in consequence of an error of observation, and not differing from each other, cannot be retained—*Lepomis* must be therefore used, as it should have been for one of the genera or subgenera, if both had been established on true principles. *Dioplites* is restricted by Dr. Girard to the species without teeth on the tongue.

The Cuvieran section of "Percoids a Joues cuuirassés" is retained as a "tribe" under the name of Cataphracti, and is divided into three families, *Heterolepidæ*, *Cottidæ*, *Scorpænidæ*. Perhaps the families so indicated are valid, but the characters given to them are vague and will require revision after a comparative study of the foreign genera.

The family called Heterolepidæ had been previously named by Swainson Chiridæ, and that name should have been retained, as well on account of its priority, as its consonance with the terminology of the other families.

In the family of Cottoids, the species are distributed into ten genera, all of which appear to be founded on good characters, but the names of some of which are objectionable.

Among the Salmonoids, the three genera of Valenciennes, *Salmo, Fario* and *Salar* are accepted, but we notice that Dr. Girard has named all the new species he describes as belonging to those genera, Salmo—"*Grd. MSS.*" in the synonymical lists of the species.

In the family of Clupeoids, the modifications of Valenciennes have not been adopted, and the genus *Hyodon* is interposed between *Meletta* Val. and *Engraulis*.

The genus *Anarrhichthys* of Ayres is adopted; its only species is called *Anarrhichthys felis* Girard. To this name we desire to draw the attention of our readers, as an important question of nomenclature is involved.

In the "Proceedings of the Academy of Natural Sciences of Philadelphia" for 1854, Dr. Girard mentions a fish which he calls *Anarrhicas felis*, and observes that two large specimens were received in such a precarious state of preservation that there was no probability of keeping them, and that having mislaid the notes, *no diagnosis could be given.*

Subsequently, Mr. Ayres, in the Proceedings of the Californian Academy, gave a full description of a species which he referred to a new genus and called *Anarrhichthys ocellatus*, which was chiefly distinguished from *Anarrhicas* by its anguilliform body, and the union of the dorsal, caudal, and anal fins. This is the species that Dr. Girard has claimed as his own *Anarrhicas felis.*

To this reference we would remark that as *Anarrhicas* has by all modern naturalists, been restricted to such species as had the dorsal and anal fins separated from the caudal, we would infer that any species placed without comment in the genus would have those characters. Dr. Girard's name of *Anarrhicas felis* was not only without any description whatever and therefore not established, but a statement by implication was made that the species possessed the stout body and fins of *Anarrhicas*, and was consequently in direct opposition to the characters of *Anarrhichthys ocellatus.* Such being the case, Dr. Girard's name cannot be adopted, and that of Ayres must be retained.

The family of *Golidæ* is limited to the species with the ventral fins united in the manner of a funnel and thus excludes the *Eleotroids* as well as the *Cyclopteroids.* The latter are very properly regarded as distinct, but the propriety of excluding the former from the *Gobioids* is more doubtful.

The genus *Gobius* is limited to the species with cycloid scales. The name cannot be retained for such species, as the genus had by several naturalists been previously restricted to species with pectinated scales. Mr. Gill has framed for the two species thus referred to *Gobius*, a genus which he has called *Lepidogobius.*

We find that in the family of Cyclopteroids, the Gronovian name of *Cyclogaster* is substituted for the Artedian name of *Liparis. Liparis* has been almost universally accepted by naturalists, and being the prior name, should be adopted. No description is given by Artedi of the generic or specific characters of *Liparis*, but the references he has given are full and ample, enabling us without doubt to ascertain what is meant.

In the Proceedings of the Academy of Natural Sciences, Dr. Girard has described a genus under the name of *Homalopomus* which he has referred to the Trachinoids. This is now referred to the Gadoids, and a doubt is even expressed whether it is distinct from *Merlangus* or *Merlucius.* It does not appear to differ from the latter genus. The cause of the former erroneous reference is attributed to the broken tips of the rays of the specimen on which the species was founded.

The family of Embiotocoids is rich in generic forms, nine being described and illustrated, and references being made to the descriptions by Dr. Gibbons of five others which Dr. Girard was unable to identify with his. Some of the species described as new by Dr. Girard will probably be found to have been previously indicated by Gibbons.

In the remarks on the family, no mention is made of the presence of the two rows of lamellæ which are present on each of the branchial arches, and which was especially noticed by Prof. Agassiz. This character has been regarded as having considerable importance by many ichthyologists and being one of the best distinctive ones of the family, the fourth branchial arch of the Labroids having but one row of lamellæ.

Dr. Girard claims "that the real knowledge of the remarkable peculiarities concerning some of their habits was obtained in the spring of 1852, by Dr. Thomas H. Webb, while attached to the United States and Mexican Boundary Commission."

The genera described and illustrated are *Embiotoca* Ag. with seven species, *Damalichthys* Girard, *Phanerodon* Grd., *Abeona* Grd., *Rhacochilus* Ag., *Hysterocarpus* Gibbon, *Holconotus* Ag., each with one species, *Ennichthys* Grd., and *Amphistichus* Ag., each with two species.

These genera are chiefly characterized by the comparative size of the head; the character of the lips, and the attachment or non-attachment of the lower one by a frænum to the jaw, the comparative protractility of the premaxillaries; the number of rows of teeth on the jaws, and the outline of the dorsal fin.

Dr. Girard has given some information on the embryology of the Embiotocoids, which will prove of general interest. He denies the presence of any resemblance between their gestation and that of the marsu-

pial mammals. Want of space forbids us to make extracts from the recorded observations, and we must remain satisfied with referring to the text and plates of the report.

Of the Cyprinoids, numerous genera and species are described from almost every portion of the west. The family is divided into the tribes of 1. *Cyprini* with teeth of the molar kind, of the grinding type, 2. *Catastomi*, with pectiniform teeth; 3. *Chondrostomi*, in American species of which there are no barbels, and the teeth are of "the grinding type and cultriform kind;" 4. *Pognichthyi*, in which barbels are present, the snout is prominent, and the pharyngeal teeth are more or less hooked; and 5. *Alburni*, which differ from the *Pogonichthyi* by the absence of barbels.

The author has devoted much time and research to these groups, and all of the genera may be good, but some of them appear to be distinguished on quite slight grounds, and many of the species are small and perhaps the young of others. But on this question we will not venture to disagree with Dr. Girard.

Most of the new genera have received Indian names, and although not cacophonous, some of those names appear strange to ears that have been mostly accustomed to Latin and Greek derivatives.

In *Dionda*, a genus of Cyprinoids, Dr. Girard has named two species collected by Capt. John Pope, *Dionda episcopa* and *D. Papalis*.

The genus "*Argyreus* Heckel" is synonymous with *Rhinichthys* of Agassiz. It is not probable that Dr. Girard will be sustained by Ichthyologists in this application of Heckel's name. An extract from the remarks of the learned Doctor himself on the nomenclature of the genus will show the history of the name *Argyreus*.

"Heckel includes in this genus two species which are generically disdistinct *Cyprinus atronasus* Mitch., and *Cypr. rubripinnus* Mus. Par. MS. But *Cyprinus rubripinnis* is identical with *Leuciscus cornutus*; and since *Leuciscus cornutus* is to enter the genus *Plargyrus* of Rafinesque, *Cyprinus atronasus* remains as the type of the genus *Argyreus* which again is identical with *Rhinichthys*. It must be recollected, however, that the teeth figured by Heckel under the name of *Argyreus rubripinnis* are those of *Plargyrus cornutus*."

The diagnosis and illustration of *Argyreus* having been by Heckel founded on "*Cyprinus rubripinnis*" and not answering to *C. atronasus*," the former species in the type of the genus, and if Rafinesque's name *Plargyrus* is adopted, Heckel's name must be treated as a synonyme. Heckel would scarcely have referred a species of *Rhinichthys* to the same genus as *C. rubripinnis* if he had known the pharyngeal teeth, and he could only have done so from an ignorance of the species. Because a species on which a genus is established belongs to a previously named genus, it by no means follows that the generic name has to be used for another species of the genus, when it proves to be distinct from the type.

If the above views are correct, *Rhinichthys* will have to be retained for the genus called by Girard *Argyreus*, and the two species described by him in his report must be called *Rhinichthys dulcis* and *R. nubilus*.

The Lepidostei have been separated apparently on good grounds into three groups distinguished by the comparative length and breadth of the

snout, and the presence of one or two rows of teeth in the upper jaw. For two of these groups, Rafinesque's names of *Cylindrosteus* and *Atractosteus* have been adopted, although to Dr. Girard, the credit of first giving them valid characters is due.

In the Plagiostomes, we have some interesting additions. A second species of *Triakis* (*T. semifasciatus* Grd.) is described. Probably this is the species noticed as *Triakis californica* in the list of Chondropterygii of the British Museum, but as this name is unaccompanied by a description, Girard's name must of course be retained. A new species of *Heterodontus* is described which is called *Cestracion francisci* Grd. But the name *Heterodontus* of Blainville must be retained for the Cuvieran *Cestraciontes*, and the species must be consequently called *Heterodontus francisci.* The *Notorhynchus maculatus* of Ayres is referred to the genus *Heptanchus*, Raf.

Among the Rays, a second species of Muller and Henle's genus *Uraptera* is made known.

Among the Cyclostomi we perceive that Dr. Girard has not only retained the genus *Ammocœtes*, but he has even separated from it a new genus which he has called *Scolecosoma.* The researches of A. Muller have demonstrated that the *Ammocœtes* are only the young of Petromizontoïds, and there is no reason to doubt his accuracy. Dr. Girard must have been acquainted with these researches, and it would therefore have been more advisable not to have added to the number of names, until it was certain, as may possibly be the case, that there are fishes of the *Ammocœtoid* type which are adult.

We have now concluded, and although we think that there is cause to dissent from the author in many cases, we most cheerfully bear witness to to the general ability with which the work has been performed, and to the very great advance in our knowledge of the Fauna of our Pacific possessions.

To the Smithsonian Institution, we are indebted for the accumulation of the materials which have been used in the elaboration of the report. Although published by the liberality of the General Government, it is one of the valuable "contributions to knowledge" which we owe to the fostering care of that Institution. T. G.

IV. ASTRONOMY AND METEOROLOGY.

Solar Eclipse of July 18, 1860.—From the numerous accounts which have reached us of the observations made upon this Eclipse we place the following before our readers.*

1. *Notice of the Astronomical Expedition to Cape Chudleigh* (or *Chidley*), *Labrador*, (in a letter to one of the Editors).—*Dear Sir:*—The American Astronomical Expedition despatched to the coast of Labrador for the purpose of observing the total solar eclipse of July 17th, (astr. time), 1860, sailed from the Navy Yard at Brooklyn on the morning of the 28th of June last. The expedition had been organized by the accomplished and energetic Superintendent of the U. S. Coast Survey, under

* For LeVerrier's account of the French Expedition, see *Postscript*, p. 309.

authority of joint resolutions of Congress passed at the late session; and consisted of the following gentlemen, viz: Prof. Stephen Alexander, of the College of New Jersey, Prest. F. A. P. Barnard, of the University of Mississippi, Lieut. E. D. Ashe, R. N., director of the Quebec Observatory, Prof. C. S. Venable, of the College of S. Carolina, and Prof. A. W. Smith, of the U. S. Naval Academy at Annapolis. With these were associated the Commander of the Coast Survey steamer *Bibb*, conveying the expedition, Lt. Alexander Murray, U. S. N. Messrs. P. C. Duchochois, of New York, and J. P. Thompson, of the Coast Survey service, accompanied the corps for the purpose of taking photographic impressions of the eclipse; and Mr. W. A. Henry, of Washington City, attended as assistant to the chief of the corps.

In addition to the purely astronomical objects of the expedition, advantage was taken of the opportunity it presented, to secure determinations of the important magnetic elements at the observing station, as well as meteorological and other observations continued throughout the entire period of absence. The thermometrical and barometrical observations were kept up hourly from the time of sailing until the morning of the day on which the Bibb entered the harbor of Newport. On the day of the eclipse the intervals were reduced to a half hour. Frequent record was made of the surface and deep sea temperature of the water.

The magnetic observations were placed under the charge of Messrs. Edward Goodfellow and Samuel Walker, of the Coast Survey. The meteorological, under that of Prof. Venable, of the astronomical corps, assisted by Oscar M. Lieber, Esq., of South Carolina. Mr. Lieber also undertook such observations of the geology of the coast as the opportunities afforded would permit.

The history of the expedition and its results have been communicated by Prof. Alexander, the chief of the corps, to the Supt. of the Coast Survey, and will probably be, at a suitable time, presented to the public in full. The present memorandum is furnished by request, and with the permission of the Superintendent, with the view of presenting in concise form the facts of most immediate interest to the scientific public.

The track of the central eclipse left the eastern coast of Labrador in lat. 59° 51½′. On the evening of the 13th July, the expedition had reached this parallel; and was, for several hours, engaged in the endeavor to penetrate the seemingly unbroken and gloomily frowning barrier of precipitous cliffs which marked this rock-bound shore. The navigation at this point was of the most hazardous character. It was necessary to feel every foot of progress with the lead, and the successive soundings, which gave everywhere, a rocky bottom, showed the most singular and sudden variations of depth. One cast, for instance, would give ten fathoms, the next, no bottom with nineteen, and the next again, seven or eight fathoms. On every hand were seen rocky islets, rocks nearly submerged, or reefs and breakers.

Through such a sea, for five or six hours, from six o'clock till nearly 12 at night, the Bibb was engaged in cautiously seeking out for herself a harbor of refuge; and just about at the moment when the sun was passing his lower culmination, though a bright twilight still filled the atmosphere, she dropped her anchor in the inlet which divides Au-

lezavick Island from the main land of Labrador. This was the point which had been previously assumed to be fittest for the purposes of the expedition; but so imperfect and even erroneous had all the charts of the coast been found to be, that even the existence of the island was regarded as doubtful; and the identity of the inlet was not fully recognized until further celestial observation and some hydrographic exploration and survey had established it.

During Saturday, July 14, a location was fixed on for the encampment, and some progress was made in the shore arrangements. The harbor, once entered, proved to be commodious and secure; and it was found practicable to moor the *Bibb* so near to the land as to enable a portion of the party to sleep on board of her, and thus to avoid some of the discomforts of camp life, which, in a region so bleak and dismal, are by no means trifling. Others, including the meteorologists, the magnetic observers, and the members of the astronomical corps in charge of the transit instrument, were compelled to make a larger sacrifice of their ease. What that amounted to may be appreciated, when it is mentioned that several of the tents were blown down almost immediately after their erection; and that a wind as disagreeable for its piercing chilliness as for its force prevailed with little intermission during the entire stay of the expedition, amounting to eleven days. The tents when reërected, were secured, or anchored, by piling rocks upon the margin of the canvass.

The exact latitude of the observing station was a few seconds short of 59° 48′; the longitude, by chronometer, 4^h 16^m 53^s west from Greenwich.

Hardly had the landing been effected when there commenced a storm of wind and rain which rendered any attempt to use the transit instrument impossible for two or three days. Even up to the Tuesday night preceding the eclipse, the clouds had been so persistent as almost wholly to prevent the observation of the stars. On that evening the sunset seemed to hold out better promise for the coming night and the ensuing day. But on Wednesday morning the prospect for the day was more than doubtful. Fleecy cumulus clouds made their appearance in numbers constantly increasing; and at the moment when the eclipse began, the sky was more than half covered. The sun was, however, totally unobscured at the beginning and for a large portion of the time during the progress of the eclipse. Clouds were, however, continually drifting over it, concealing it for brief intervals entirely from view. At the very close, the alternations of sun and shade were so rapid, that it became a question of doubt whether the final contact would be secured, and a flutter of excitement prevailed throughout the observing corps: but the last four or five seconds presented the sun's eastern limb entirely unobscured; and the desired point was satisfactorily gained.

Just previously to the time of the sun's total immersion, a thin veil of cloud intervened between it and the observers, not dense enough to intercept the direct rays of the luminary, but too dense to allow the corona surrounding the dark moon during total obscuration to be visible. Lt. Ashe was fortunate enough, however, to catch one point of brightness and to fix its position in this corona; and this may serve a useful purpose hereafter, in corroborating observations elsewhere made,

under more favorable circumstances, in regard to the features of this beautiful and rare phenomenon. The bright point observed by Lt. Ashe was white and not ruddy. The expedition are unhappily unable to bear any testimony in regard to the roseate clouds which have been so often seen during total eclipses upon the moon's border. This has been a subject of great regret, the more so because the corona which was lost to the astronomical party, was not lost to such of the ship's company as remained on board of the Bibb. From the description given of it by these, the chief of the corps has prepared a drawing, in which there appear four principal radiant beams extending outward beyond the general limit of the coronal luminosity, in positions sufficiently well fixed to admit of comparison with observations made elsewhere. But as this class of observers were not furnished with instruments, they gave no testimony as to the positions, or even as to the presence of rosy clouds.

The whole astronomical corps observed the breaking up of the last line of solar light lingering before total obscuration, into the fragments commonly called "Baily's Beads," from Francis Baily, President of the Royal Astronomical Society, by whom they were described in the Mem. Astr. Soc. for 1837, as observed by him in the annular eclipse of 1836. These fragments were very evanescent, and were not preceded by those longer dark filaments or ligaments noticed by Mr. Baily on the same occasion, and more or less perfectly by others since. At the emergence of the sun, the beads were not noticed, owing probably to the veil of clouds. Only two of the observers attempted, in fact, to fix the exact second of emergence.

The darkness which prevailed during total obscuration was not as remarkable as had been anticipated by most of the observers. The present writer, for instance, found no difficulty in making pencil notes at this time, or in reading lines written in pencil in other parts of his note book. It was not necessary to bring the book nearer to the eye than usual.

The pallor or ghastly appearance which has been remarked at such moments in the human countenance by former observers, did not strike the members of this party, though it was looked for.

There was something indeed about the character of the gloom which was unusual and impressive, but it scarcely effected the tints of objects or rendered the face of nature very different from what it appears during early twilight. Clouds covered at the time almost nine-tenths of the heavens; and in the intervals of the clouds the blue of the sky was intensely deep and dark. On one side only was the horizon unobscured. This was on the north where the harbor opened out to the sea; and in this direction a beautiful rose and orange flush presented itself.

It would be easy to extend this notice to much larger dimensions, if space in the present number of the Journal was available.

The instruments employed in the astronomical observations, were a thirty-inch transit by Fitz, a fifty-one inch Fraunhöfer achromatic belonging to Princeton College, a forty-two inch by the same maker belonging to Columbia College, one of similar dimensions belonging to Lt. Ashe, a thirty-inch equatorial belonging to the Naval Academy at Annapolis, a three foot alt. and azimuth, belonging to the College of

South Carolina, and a mammoth comet-seeker, of 7½ inches aperture, by Fitz. An equatorial belonging to Mr. Rutherford of New York city, a gentleman well known for his disinterested zeal and efficient labors in the cause of astronomical advancement, served the photographers of the expedition to fix from time to time the successive phases of the eclipse. Another comet-seeker was fitted up for the purpose of furnishing an image of the sun upon a white ground in a darkened chamber.

Fifteen auroras were observed during the absence of the expedition. In nearly every instance a corona was repeatedly formed, though many of the auroral clouds were exceedingly filmy and thin.

Atmospheric electricity was almost or absolutely *nil* during the entire stay of the expedition at the observing station. The passing of the shadow seemed to produce no change in this respect.

The diurnal magnetic variation was very large, varying from two to five degrees. During the eclipse the needle was more quiet than before or after. The surface temperature of the sea was very low from the Straits of Belle Isle northward. It was frequently down to 38° or 39°. But the lowest surface temperature at any time observed, was in the Straits themselves, on the return passage; when the thermometer marked, in the surface water, 32°.

In the harbor at the observing station, ice formed in the shoals near the beach on the 13th of July. On Sunday, July 22, there occurred a storm of snow and sleet, which covered the deck of the steamer, and wrapped the whole surrounding country so far as visible, in a mantle of white. On the return passage, this snow was observed still enveloping the mountains far down the coast.

Scarcely any quarter of the world presents, perhaps, more difficult or dangerous navigation than the coast of Labrador. The islands, islets, submerged rocks and reefs are absolutely innumerable, and icebergs swarm where these more fixed dangers are wanting. The month of July was pronounced by the hardy mariners engaged in the Labrador fisheries, to have been one of the most tempestuous ever known in those seas, and most prolific of disaster to their fishing vessels. It will therefore occasion no surprise to state that the Bibb has been repeatedly in positions of hazard, requiring all the resources of her officers to meet successfully. The members of the corps cannot but feel that the hand of a protecting Providence has been more than once distinctly visible in preserving them amid dangers, and delivering them from situations, to which they can hardly look back with tranquillity. F. A. P. B.

2. *Extract of a letter from the Superintendent of the Coast Survey to the Editors in relation to observations made on the Western coast of the United States, for the Coast Survey*, by Lieut. J. M. Gilliss, U. S. N.—Lieut. Gilliss arrived at the station selected by him for observing the eclipse, and which is near Steilacoom, Washington Territory, on the 9th of July. Here he encamped and made his preparations for observations of time, latitude, etc. These are not yet definitely worked up and hence I do not give the position of the station or the times of the different phenomena in detail at present. The following particulars from Lieut. Gilliss's report will be found of interest in anticipation of the time and longitude results:

"For the first time after our arrival at the station, the sun rose clear on the morning of the 17th, nor was there at any time during that day more than two-tenths of the sky obscured by clouds. Yet, although the evening was absolutely cloudless, and the stars were shining with remarkable lustre after midnight, so fickle had been the climate during the preceding three weeks, that when we closed the tent, three hours before the eclipse would begin, I had no confidence that the next morning would be favorable for observation.

By $3\frac{1}{2}$ A. M. we were up and had removed the meteorological instruments from camp to the knoll. At that time it was sufficiently light to write without artificial aid. Mt. Rainier was distinctly visible and sharply cut against the southeastern sky. Beyond it and towards the point at which the sun would rise, there was a stratum of vapor whose upper line was slightly inclined from mid height of Rainier towards the northern horizon. At that time the barometer stood at 29·698; att. thermometer 44°·5, the temp. of the air 45°·2, and there was only a very slight air from the southward. At ($0^h\ 17^m$, sid. chron.) the mist striæ became dense to the N. and E., and were more evidently in rays diverging from the point of sunrise to an elevation of some 25°. The air was so cool and so loaded with moisture that although the telescope had been out all night the object glasses were densely covered with dew immediately after the caps were removed. By ($0^h\ 30^m$) a part of the vapor in the N. and E. had condensed into little cumuli beyond the Cascade range each more light and feathery with distance from diverging point, though none of this series extended as far as Mt. Rainier, and it was only towards the north that a dense volume of vapor could be seen coming in towards the lower lands bordering on Puget's Sound. Two minutes later and the edges of the little flocculi were tipped with pink and golden hues increasing in brilliancy of color as the sun approached the horizon.

The eclipse had far advanced when the first cusp appeared above the horizon at ($0^h\ 39^m\ 38^s$). It was seen through a red screen glass and was sharp and without tremor. Indeed the atmosphere was so still that the rise of the second cusp over the distant ground line at ($0^h\ 40^m\ 58^s$) was observed almost with the precision of a transit of a limb over the wires of a telescope. But it was at once perceived that there was great disturbance of the lune, the lower half being flattened by the unequal refraction.

At this time I was again obliged to wipe the heavy drops of dew from the object glass of the telescope, and whilst so doing my attention was directed to the vapor near us. The whole northeastern portion of the prairie had apparently been converted into a placid lake with here and there a knoll projecting through and forming a minature isle, the illusion being enhanced by rapidly diminishing intensity of the light. At ($0^h\ 54^m$) distant objects could not be recognized more distinctly than during midsummer twilight at $8\frac{1}{2}$ P. M.

At ($0^h\ 55^m$) the southern cusp had become rounded off and rugged as though the moon's edge was serrated. But had such been the case this portion of the lune would have broken into beads of light before the total obscuration took place, and that did not occur, the moon's disc equally and uniformly interposing between us and the sun until the last glimmer of light disappeared.

I had turned off the red screen half a minute before and was surprised to behold quite distinctly the following segment of the lunar sphere. The periphery of this segment was more than 100°. Its color was uniformly shaded from an intense black at the centre of the lunar disc to a very dark grayish purple near the limb of the sun. It was still traceable during twenty seconds after the last glimmer of sunlight.

At the moment of totality beads of golden and ruby-colored light flashed almost entirely around the moon not constant even for a second at one point but fitfully flashing as reflection from rippled water, and as mutable in the respective places of the colors. This bead-thread could not have extended more than ten seconds beyond the lunar disc. It broke up suddenly at ($1^h\ 16^m\ 21^s.2$ sid. chron.) and then for the first time protuberances were noted beyond the following limb of the moon. The position of its largest one was 75° or 78° W., and in the form of a flattened cone or pyramid of cumulus cloud about one minute in height and when first observed perhaps two minutes broad at base. The cloud was not a uniform mass but apparently an aggregation of small ones, and its general tint was a rosy pink with occasional spots and edges of yellowish white light as though sunlight shone obliquely through them. Except in the pink color it greatly resembled the protuberance noted during the total eclipse of the sun observed at Olmos, Sept. 7th, 1858. As the moon advanced this protuberance was probably broader at the base and brighter at the summit while its apparent elevation remained the same. This was an extremely beautiful sight, and I watched it closely, giving nearly all my attention to it during 15^s, yet at the same time I was able to perceive a lesser one of a more flattened appearance distant 10° or 15° towards the west, and several others yet smaller and one long one of a much darker color at different points of the disc. Intently occupied with the great protuberance, the corona had not been recognized up to this instant ($0^h\ 58^m\ 10^s$), interest in the former causing me also to drop the beat of the chronometer.

It was then so dark that I found it impossible to recognize the second's dial of the chronometer (the gold one) and Mr. James Gilliss was called to bring his lantern and read the time at which I should indicate the second internal contact of the limbs. Raising my face from the box on which the time keeper stood to the telescope a most extraordinary scene was apparent! Over the moon's black disc colors of the spectrum flashed in intersecting circles of equal diameter with that body, and each apparently revolving towards the lunar centre. The moving colors were not visible beyond the moon, but a halo of virgin white light encircled it, which was quite uniformly traceable more than a semi-diameter beyond the black outline. This corona was composed of radial beams or streamers, having slightly darker or fainter interstices rather than a disc of regularly diminishing or suffusing light; but the gorgeous appearance of the spectrum circles with their incessantly changing bands of crimson, violet, yellow, and *green*, thoroughly startled me from the equanimity with which the preceding phenomena had been observed. Nor were these colors physiological results from a change of position of the body, or of preceding strain of sight in efforts to recognize the division of the second's dial, in darkness, and subsequent direction of the eye towards the

sunlight, for they continued visible with the telescope at least 10^s longer. As near as it was posible to estimate, the breadth of each spectrum circle was about two minutes. The green colors were not darker than the tint usually called pea green, and were on the edges farthest from their respective centres, but neither of the lines seemed to retain a definite position, and I was irresistibly drawn to their contemplation to the neglect of all the changes that might have been taking place in the protuberance and corona.

They vanished with the first appearance of sunlight beyond the western limb of the moon, their sudden obliteration causing me to utter an exclamation which was regarded as the signal for noting the time, a datum whose importance had been wholly forgotten in the fascination thus caused. I cannot liken them to anything so nearly as to the image seen in the kaleidoscope."*

3. *Observations made during the Total Eclipse of* 18*th July*, 1860, *on the summit of Mount Saint-Michel, in the Desert of Palmas, Spain;* by A. SECCHI, S. J. Communicated to the French Academy of Sciences. (*Comptes Rend.*, li, p. 156, July 30, 1860.)—The place where I observed the eclipse was on the top of Mount Saint-Michel, in the desert of Palmas, at the same point chosen by Biot and Arago for their triangulation operations, and at a height of 725 metres (=2378 ft.) above the sea, commanding a very wide horizon. The weather was magnificent during the whole time of obscuration, notwithstanding we were tormented with a cruel anxiety up to a few moments of the time by parasitic clouds forming continually on the mountain and dissolving only when at some distance. But they happily disappeared just before the critical moment, and the sky was fine till evening.

I was accompanied by M. de Aguilar, director of the Madrid Observatory, and by Mr. Cepeda, lawyer from Valencia, a distinguished amateur.

The instruments were a Fraunhöfer telescope of 78 mm. aperture (about 3·04 inches) and $1^m{\cdot}20$ (=$47\frac{1}{4}$ in.) focal length, with powers of 60, 90, and 130 diameters. The two first powers gave the entire disc of the sun, and the three oculars being mounted on a slide could be changed with the greatest rapidity. The micrometer carried a system of six spider lines, with spaces of 6′ (invisible in the dark), and of four very fine platinum wires so disposed that there was a space just equal to one lunar diameter between the outer wires—the two intermediate wires were slightly inclined, subtending 1′ 30″ at the narrower, and 2′ 30″ at the larger angle; an arrangement designed to aid in obtaining a more exact estimate of the protuberances. The whole micrometer revolved on a transom, with a plate on which was a graduated circle and a sheet of white drawing paper upon which the angular position was marked by touching a pencil, carried by the fixed transom—thus reserving the reading until the observations were concluded. This instrument was mounted equatorially, was very stable, and had been adjusted the day before.

* Up to this date, Sept. 7th, the party sent to the Cumberland House, British Columbia, have not been heard from.—EDS.

Some minutes before the commencement of observations I verified the position of the telescope and the commencement was marked by a Morse telegraph, kindly procured from Madrid by M. Aguilar, provided with a pendulum which marked the seconds. A simple mechanism marked the instant of observation. Some minutes after the commencement I sought for the disc of the moon outside of the sun but could not make it out. At $2^h\ 19^m$ I succeeded in seeing very clearly through an arc of about 10° or more, but some time after the moon disappeared, and after that it could be observed only for an instant. Is this due to the inequality of the portions of the solar corona, upon which the disc of the moon is projected? I observed with certainty that not only the edge of the solar crescent was more sharply defined upon the side of the concave phase, than upon its own proper border, but also that the field of the telescope was much more clear upon this side than upon that of the moon, and the same could be seen in projecting the solar image on white paper.

The cusps remained throughout very distinct, and the solar spots were successively eclipsed without any distortion as viewed with a magnifying power of 90. The lunar mountains were well outlined upon the solar ground, and indented the inner border of the limb. After the centre of the sun was hidden (and even a little more) the light of the horizon diminished suddenly in a decided and unexpected manner. Surrounding objects did not however noticably change color. As the eclipse was becoming nearly total, I took away all the fixed colored glasses, and followed the sun with a glass held in the hand. This was an excellent glass of neutral tint made by Lerebours, a graduated light, the lighter shade being very delicate. The slender crescent is now breaking into many parts near the cusps which still remain very distinct, and the corona begins to show itself even with the dark glass. The sun reduced to a simple thread disappeared without forming (*grains des chapelet*) Baily's *Beads*. Quickly taking away the colored glass I was surprised to see the sun yet white and its light so strong that it hurt my eyes, but its splendor visibly diminishing and changing to a purple light, which at once appeared to terminate in an infinity of purple points, which were as soon hidden, and then two great red protuberances appeared near the point of occultation. One of these was at least 2′ 30″ in height, and as large at the base as 2′; it was conical in form, slightly tapering, curved at the top. The other was about half the height of the preceding, extending over a considerable arc, at least 5° upon the solar border. Its top was formed like very minute saw teeth, parallel to the border of the moon. I looked as soon as possible at the opposed margin of the sun, but nothing appeared. Returning to the first margin I saw that the protuberances were rapidly hid. During the whole time the corona was magnificent but most brilliant on the side on which the eclipse began. Its light was all around uniform and without interruption, of a beautiful silver white, shading off gradually from the margin of the moon to the distance of about the lunar radius or less. At this distance there began to be many interruptions; large sheafs of light appeared, those of the upper part were the largest, and extended out to a distance equal to a diameter and a quarter of the moon. On

the lower part I saw only one of these long sheafs. With Arago's polariscope, already directed very near the sun, I ascertained that the two images were not of equal brilliancy, and that the corona in one was lengthened in one direction, and in the other in a direction perpendicular to the first, but I could only give some seconds to their examination.

Returning to the telescope, I regarded for an instant the imposing scene which was then displayed in all its majesty. The moon perfectly black showed itself with all the glory of its rays, which appeared lengthened below and to extend out to the distance of two solar diameters. The heavens were of a light ash color, but not of threatening aspect. Near objects were plunged into feeble twilight contrasting with distant objects not yet in shadow. All this unique scene remains profoundly engraven on my mind; the solemnity of the spectacle appeared forcibly to impress the assistants, who, though numerous, all remained in perfect silence. Not to lose these precious moments I returned immediately to the telescope. The aspect of the sun was much changed. The two great protuberances of which I have spoken had already disappeared, and a great number of others appeared from on all sides of the sun (this moment corresponding to the middle of the total obscuration), I was for an instant embarrassed to decide which to select for measurement of their angle of position; for it was useless to measure the size, which changed while looking at it. With the mechanism of the micrometer in a few seconds I determined six—although I counted at least ten—there was hardly any part of the surface of the disc where there was not a point, they seemed regularly distributed. These are the angles taken in reckoning from east to northwest. South: 39·0°, 75·0°, 116·0°, 173·0°, 211·3°, 310·0°.

A greater brilliancy of the corona on one side announced that the sun was emerging, then in directing my attention to this side I was astonished to see a very large number of very small protuberances, and above all of them a red cloud entirely detached which was suspended and separate from the rest and from the lunar margin by a marked white space. Its figure was elongated, about 30″ of length to 3″ of width, and its form somewhat tortuous and sharp at the extremities (I called to my companions to witness this). This cloud was not alone. I had the conviction that it was accompanied with many others which rested at nearly the same level as a series of cirrus. Their color was that of the protuberances, only a little more distinct.

During all this the number of protuberances increased greatly upon this side and soon assumed a continuous arc formed like a saw, which extended at least to 60° of the circumference, and which gradually lengthened while its central part increased in width and brilliancy. The purple color mingled with the white in gradual transition until the white became so strong that the eye could not sustain it; the protuberances then disappeared.

The sun then began to shine in the heavens like a point of true electric light, and made a singular contrast with the corona still remaining, and which (by hiding with my hand the bright part) I could see for 40 seconds longer. That which most struck me in the circumstances, was the immense quantity of red protuberances and their distribution; so

that one could absolutely say they enveloped the sun. Those commonly observed are only the summit of the most elevated, and without doubt it is only in certain favorable circumstances that we can see the sun entirely crowned by them. This corona of light prevented accurate observations of time, and it would give a different solar diameter according to the depth of shade of glass employed.

The time of total obscuration was found by M. Cayetano de Aguilar to be $3^m\ 11^s$, but it passed like a moment and seemed to us, at the most, not more than two minutes.

My convictions upon the nature of that which I saw are that the phenomena were real and that I truly saw the flames in the solar atmosphere and clouds suspended in these flames; it would be impossible to imagine anything else, as for example, that it might be some phenomena of diffraction or refraction.

The clear graduation and distinct mingling of the peach blossom colored light with the white photosphere was of a character so distinct that it can never be mistaken by any phenomena of interference, of refraction, or any illusion whatever. I do not doubt that it really appertains to the sun, and the structure of these suspended clouds tends to strengthen my conviction. As regards the part of the corona more remote and those long bundles of rays, the thing does not appear to me so certain: they have too much the aspect of those seen through the clouds at sunset. Yet it is important to distinguish from these the true corona which was continued much beyond the protuberances.

M. Cepeda, however, who made his observations with an excellent telescope, having a large field, assures me that he saw a bundle of rays, curved and branched like the horns of a stag, at the upper part.

All these observations have been confirmed by photography.

The director, M. Antonio de Aguilar, had brought, for making the solar photographs, the large telescope of Cauchoix, mounted upon a solid cast iron foot, furnished with clock-work. Besides numerous proofs of the entire sun, he took 14 impressions on a larger scale, and 5 of the natural size of the focal image, 23 millimeters in diameter, and which represent all the phases of the phenomena. The examination of these photographs will be made under more favorable circumstances with proper instruments. At present, I will only say that the times of exposure varied from from 3^s to 30^s, that all the images are solarized in the protuberances, but the corona has an intensity differing according to the time. There was not the same intensity throughout but the most vivid parts do not correspond to the protuberances.

We notice also a greater intensity in the chain of protuberances toward the first and the last instant of total occultation. The force of the light of the protuberances is such that one impression is become triple by a momentary jar of the telescope. In this delicate operation M. Monserat, Professor of Chemistry in the University of Valentia, was charged with all photographic operations, and my compeer, P. Vinader, took charge of the regulation of the telescope. This communication has already become so long that I omit ordinary observations, and will only say that the light was strong enough to enable one to distinguish small objects, and to read without difficulty ordinary books, and without

seeking I saw Jupiter, Venus, and some other luminaries. A portion of this light may have proceeded from the reflection of a thunder cloud, a short distance from and feebly lighted by the sun.

I give the result of the observations made with the thermomultiplier of Melloni by M. Botella, inspector of mines. In general the progress was very regular, as the figures show:

Commencement,	1h.	57m.	Galvanometer,	20·0
	2	11	"	18·3
	2	25	"	15·5
	2	35	"	11·5
	2	58	"	2·0
	3	5	"	1·5
Totality,	3	10	"	0·0
Emergence of the sun,	3	12	"	0·5
	3	20	"	1·0
	3	35	"	12·0
	3	55	"	15·0
	4	16	"	17·5
End,	4	30	"	20·0

A very sensitive declinometer of Jones, observed hourly by M. Mayo, engineer, showed no disturbance. Professor Barreda observed the solar spectrum at my request, and will give his report thereon in a special memoir.

4. *On the polarization of the light of the corona, and of the red protuberances, in total solar eclipses.*—M. Prazmowski observed at Briviesca in Spain, with special reference to these subjects, the total eclipse of July 18th. His observations seem to justify the following conclusions, viz.

(1.) The light of the red protuberances is not polarized. In this respect they resemble clouds in our atmosphere. May we hence conclude that these are solar clouds, composed of particles, not gaseous, but liquid or even solid? The high temperature of the sun leads us to infer that these clouds are constituted of very refractory matter.

(2.) The polarization of the corona proves that its light emanated from the sun and was reflected. The bright, very decided, polarization, proves also that the gaseous particles from which it was reflected send the light to us reflected nearly at the maximum angle of polarization. For a gas this angle is 45°; but in order to reflect light at this angle it must be near the sun. A solar atmosphere seems to furnish the necessary conditions.—*Comptes Rendus, Août* 6, 1860.

5. *Baily's Beads.*—Mr. Lespiault, who watched especially for this phenomenon, says—(Compt. Rend., li, 221)—some seconds before the first interior contact, the margin formed by the arc of the moon appeared irregular and trembling, but I did not see either the "*Baily-Beads*" or "*comb-teeth.*"

6. *Third Comet of* 1860.—A brilliant comet, with a tail several degrees long, was seen by many persons in different parts of our country, on the evening of June 21st and 22d, 1860. It was seen on the evening of June 20th, by Prof. Caswell of Providence, then on the deck of the steamship Arabia. The first public notice of the comet appears to have been made by Mr. C. W. Tuttle, assistant in the Harvard College Observatory. The comet continued vissible to the naked eye about two weeks.

The following parabolic elements of its orbit were computed by Mr. Tuttle, from observations at Cambridge, Mass., of 21st, 24th and 27th of June.

Perihelion passage,	1860, June 16.06730.	
Long. of perihelion, - - -	161° 34′ 56″	Mean eqx.
" " asc. node, - - -	84 41 20	Jan. 0.
Inclination, - - - -	79 18 11	
Log. of perihelion distance, -	9.46687	
Motion, - - - - - -	*direct.*	

Gould's Astron. Jour., No. 136.

7. *The Meteor of July 20th,* 1860.—This remarkable meteor was visible over a portion of the earth's surface at least a thousand miles in length (from N. N. W. to S. S. E.) by seven or eight hundred in width; or from Lake Michigan to the Gulf Stream and from Maine to Virginia. The newspapers have contained many notices of its appearance as seen at various places within these limits, but most of these accounts are too vague to be of any scientific value. We are not yet in possession of a sufficient number of good observations for a final discussion of the phenomena presented, and can only at this time notice briefly a few of the best that have come to hand, and state some approximate results derived from them respecting the height of the meteor above the earth, the direction of its path, &c.

At New Haven, it was seen, during a portion of its flight, by several members of the Scientific Faculty of the College at the house of Prof. J. A. Porter, and pains were at once taken to fix its apparent path by reference to parts of the building, tree-tops, stars, &c., near which it had been seen, and also to determine its time of flight, by noting the time required to repeat the various acts performed while it was in sight. The bearings and altitudes of the points noted for fixing the path were subsequently determined instrumentally. Independent data of the same kind were also obtained by going with many different observers to the places occupied by them at the time, and observing with compass and quadrant the path in the sky pointed out by each, and noting the time for each in the manner already indicated.

By laying down these bearings and altitudes on a globe, a normal or average path was obtained, which cuts the horizon at N. 62° W. and S. 62° E., and gives a maximum altitude of 53°, in a direction S. 28° W.

The time of flight for the different observers, determined as above stated, ranged from 10 to 20 seconds—giving an average of fourteen or fifteen seconds, which agrees with the careful estimate made at the time by the observers at Prof. Porter's.

Valuable observations have also been received from individuals in different places, some items of which we proceed to state. They will be given more fully hereafter.

Mr. J. D. Lawson, of New York, saw the meteor from the corner of Fourth street and Broadway, and has furnished data which give for maximum altitude (N.) $56\frac{3}{4}$°. Another independent observation at the same spot, as published in the *Journal of Commerce*, gives from data subsequently obtained by Prof. H. A. Newton, an altitude of about 55°. We use for N. Y. 56° as the mean of the two.

Mr. F. Huidekoper, of Meadville, Pa., makes the altitude at that place 39° 30′ from the northern horizon; the point of disappearance at altitude 3° 30′, and 10° 45′ S. of east; time from crossing meridian till disappearance, 10 to 12 seconds.

Mr. W. King, a surveyor at Erie, Pa., makes the altitude 44°, and point of disappearance in a cloud due east at an altitude of 22°.

Mr. S. B. McMillan, of E. Fairfield, Ohio, reports it as having been seen "moving from a point about 10° E. of N. to within as much of a due east direction," attaining an altitude of 15°.

Rev. T. K. Beecher at Elmira, N. Y., saw it pass very nearly through his zenith, and "so very close to" α Lyræ "as to quench, if not eclipse it." This star was then about 11° from his zenith, and in azimuth S. 76½° E. The meteor separated into two parts with an explosion when near the zenith.

Other observations (not now at hand), which have been used in obtaining our results, have been received from Mr. B. V. Marsh, of Philadelphia, and Prof. Hallowell, of Alexandria.

A comparison of these observations, and a few of the best that have been published, give approximate results as follows:

(1.) The vertical plane in which the meteor moved cuts the earth's surface in a line crossing the northern part of Lake Michigan, passing through, or very near to, Goderich on Lake Huron, (C. W.), Buffalo, Elmira and Sing Sing, N. Y., Greenwich, Conn., and in the same direction across Long Island into the Atlantic.

(2.) In this plane the path that best satisfies the observations is sensibly a straight line approaching nearest to the earth (41 miles) at a point about south of Rhode Island, and having an elevation of 42 miles above Long Island Sound, of 44 over the Hudson, 51 at Elmira, 62 at Buffalo, 85 over Lake Huron, and 120 over Lake Michigan. The western observations, however, which are few and imperfect, seem to indicate a somewhat greater elevation than this for the western part of the path. Possibly, therefore its true form may have been a curve convex towards the earth, resulting from the increasing resistance of the atmosphere as the meteor descended into denser portions of it. The observations made this side of Buffalo, which are somewhat numerous and many of them good, are very well satisfied by the straight path already described. Further and more accurate observations beyond Buffalo are greatly needed for determining the true form and position of the orbit, both in respect to the earth's surface and in space.

(3.) The close approximation to parallelism to the earth's surface of the eastern portion of the observed path leaves it a matter of doubt, considering the imperfection of the observations, whether the meteor finally passed out of the atmosphere and went on its way in a disturbed orbit, or descended gradually into the Atlantic. The former supposition is perhaps the more probable, especially if the path was curved, as above suggested, instead of a straight line.

(4.) The meteor exhibited different appearances in different parts of its course. It seems to have been observed first as a single body, more or less elongated, gradually increasing in brilliancy, throwing off occasionally sparks and flakes of light, until it reached the neighborhood of El-

mira, N. Y. Here something like an explosion occurred, and the meteor separated into two principal portions with many subordinate fragments all continuing on their course in a line behind each other, and still scattering luminous sparks along their track, until a point was reached about south of Nantucket, when a second considerable explosion took place, and afterwards the principal fragments passed on till lost to view in the distance. The most trustworthy observations represent the meteor as disappearing while yet several degrees above the horizon, (generally from 3° to 6° or 8°). Besides the actual changes of form which the body successively underwent, apparent changes would present themselves to each observer arising from change of direction in which the meteor was seen.

(5.) It is not easy, from the observations in hand, to determine with much accuracy the velocity of the meteor while passing through our atmosphere. The time of flight is doubtless largely overestimated by most observers, especially those unaccustomed to measure intervals of a few seconds. Timing with a watch a repetition of the acts performed during the flight of the meteor, usually reduces the interval to not more than one third, or even one fifth, of the observer's own estimate. From 15 to 30 seconds is a fair range for good observations, and probably to no observer was the meteor in sight over 45 seconds or a minute, although a minute and a half and two minutes are very common estimates. A comparison of the most probable estimates of time with the length of path observed, gives a velocity ranging from eight to fifteen miles a second. Probably 12 or 13 miles is a tolerable approximation. This, allowing for the earth's motion in its orbit, gives 26 or 27 miles a second as the actual velocity of the meteor in space. Its relative velocity may have been much greater when just entering the atmosphere, than after encountering its accumulated resistance.

(6.) The actual diameter of the luminous mass, taking its apparent diameter as nearly equal to that of the moon, (the estimate of many observers nearest its track) must have been from one-fifth to one-third of a mile. Many estimates would make it still larger. The two principal heads when passing New Haven must have been from one to three miles apart.

(7.) A report is mentioned by many observers as having been heard from one and a half to five minutes after the meteor passed. The least time in which such a report could have been heard, taking the usual constant for the velocity of sound (1090·47 feet a se cond) would be about three minutes and a half. This is a point of much interest, and needs to be investigated.

The "rushing sound" spoken of by many as heard while the meteor was passing, is of course to be attributed to imagination.

C. S. Lyman.

8. *The Meteors of August 2d and 6th,* 1860.—A meteor, rivaling in brilliancy that of July 20th, was extensively observed throughout the Southern United States on the evening of August 2d, between 10 and 11 o'clock, according to the local time. It appears to have passed from east to west vertically over Tennessee at ten minutes past ten, Knoxville time. "From three to five minutes after the disappearance of the meteor a report was heard like the discharge of an eight-pounder; which was fol-

lowed by a long—*long* rolling, reverberatory sound of more than a minute's duration." This fact is mentioned by Mr. W. C. Kane writing from Knoxville to a friend in Hartford, Conn.

Another brilliant meteor was seen in the southwest, from New Haven and New York, between half past seven and eight o'clock, on the evening of August 6th. It passed from south to north, and notwithstanding the daylight still remaining attracted attention over a wide extent of country.

9. *Further Notice of the New Concord (Ohio) Meteor, of May* 1, 1860; by Prof. E. W. EVANS.—Since writing my communication published in the July number of this Journal, on the path and height of the New Concord meteor, I have found some additional data, which I regard as important because they have been furnished by a good observer who saw the meteor under favorable circumstances. A single case of this kind is the more worthy of note because, owing to the cloudiness of the day when this meteor passed, there were but a few places from which it was seen at all. The observer referred to is D. Mackley, Esq., a lawyer of Jackson, Ohio, who at the time of the occurrence happened to be at Berlin, about six miles north east from the former place, and seventy miles from the nearest point under the meteor's path. He took pains to note all the facts as accurately as he could at the time; and he afterwards returned to the spot in order to determine more definitely the points of the compass. His testimony, in answer to my interrogatories, is substantially as follows:—

"The meteor first appeared to me at a point about 55° east of north. It moved northward in a line very nearly parallel with the horizon. When it disappeared it had described an arc of about 15°. It was in sight about 6 seconds. Its altitude was about 30°. In regard to its size, I have since looked at the sun through a thin cloud, and I think the apparent diameter of the meteor was one-half that of the sun."

These data give the meteor a height of 41 miles over the northern boundary of Noble county; a diameter of three-eighths of a mile; and a relative velocity of nearly 4 miles a second. The results agree sufficiently well with those before given.

The meteor was seen through openings in the clouds at various points along a line of 60 miles, extending from near Newport on the Ohio river to the neighborhood of New Concord. The evidence, upon the whole, does not indicate any descent of the body towards the earth between these limits, or any change in its size or appearance. From this fact, and the great height of the body, and the absence of all evidence that it was seen or heard in the northern part of the State or beyond, it seems probable that this meteor was not dissipated in the atmosphere, but passed out of it again. The shower of stones which came down near New Concord had probably been detached from the principal mass before the latter came into sight.

Marietta, Ohio, Aug. 20, 1860.

10. *Shooting Stars of August* 9–10, 1860.—Since the year 1837, at least, it has been found in the Northern hemisphere, whenever the weather has permitted observation, that shooting stars have been unusually abundant during a period of several nights in August, gradually increasing in numbers for a few days up to the 10th of the month,

and then gradually diminishing in frequency. While every other meteoric period has intermitted, this of August holds out with little change.

The observations below stated were made in the open air, from the top of the southern tower of the Alumni Building of Yale College, by a corps of observers consisting of J. W. Gibbs, Jr., Charles Tomlinson, J. W. Johnson, W. C. Johnston, H. W. Siglar, J. B. Chase, with myself. During the hour from 2^h to 3^h A. M., there were but six observers. Each meteor was called aloud, and no one was reckoned twice. Between 10 P. M. of the 9th and 3 A. M. of the 10th of August, 1860, we observed *five hundred and sixty-five* different shooting stars, distributed as follows, viz:

Aug. 9.	10^h to 11^h P. M.	N. E.	29	11^h to 12^h P. M.	N. E.	14
		S. E.	30		S. E.	22
		S. W.	35		S. W.	47
		N. W.	11		N. W.	18
			105			101
Aug. 10.	0^h to 1^h A. M.	N. E.	19	1^h to 2^h A. M.	N. E.	24
		S. E.	27		S. E.	25
		S. W.	44		S. W.	31
		N. W.	17		N. W.	17
			107			97
	2^h to 3^h A. M.	N. E.	30			
		S. E.	20			
		S. W.	65			
		N. W.	40			
			155			

The meteors were reckoned in that quarter where they first became visible, but it is of course impossible to indicate satisfactorily by words their distribution on the face of the sky. The majority of them were inferior in brilliancy to stars of the second magnitude. Many however were as bright as stars of the first magnitude, and a few rivalled in splendor Mars and Venus. Several of them left luminous trains, but none appeared to explode. Their general Southwesterly direction was evident, and as nearly as we could estimate, at least three fourths of all conformed to the radiant of former years, in the vicinity of the sword-handle of Perseus.

During our watch the sky was clear except that between 1^h and 2^h A. M. there were some clouds about the west. The moon in her last quarter embarrassed our view after 11 P. M., and doubtless concealed about one third of all the meteors which might have been seen in her absence. At 3 A. M. mist was forming rapidly, but we saw after that hour *five* meteors not included above; and while we were arranging for the watch a few minutes previous to 10^h we saw *eighteen*, making a total of 588 meteors observed by us during five hours and ten minutes. Had the moon been absent we should probably have seen 800 during this period. This is not far from six times the common nightly average for a like interval.

It is perhaps hardly worth mentioning that the Aurora Borealis was visible all night, being by turns quite active after twelve o'clock. This phenomenon has been seen here with uncommon frequency this summer, but it appears to have no special connection with shooting stars.

During the entire nights of the 8th, 10th, 11th, 12th and 13th of this month, our sky was wholly overcast. EDW'D. C. HERRICK.

Yale College, New Haven, August, 1860.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *The Fourteenth Meeting of the American Association for the Advancement of Science*, was held, August 1st–8th, 1860, at Newport, R. Island, in the old State House, dating from the Colonial times under Queen Anne. The Hon. W. H. Cranston, Mayor of Newport, pronounced an address of welcome from the city of Newport, to the members of the Association. The general enjoyment of the occasion was amply secured by the kind and abundant hospitality of the city and its citizens, leaving on all who participated the most agreeable impressions of Newport. The weather was superb, neither fog or rain marred the pleasure of the unequalled drives by beach and shore over this American Isle of Wight, and the Geologists with Mr. Hitchcock's map* of the Island in hand found the conglomerates of 'Purgatory' and 'Paradise' and the graphitic anthracite of these most overdone and contorted coal measures a fruitful theme of discussion both in hall and field.

The numbers in attendance at the Newport meeting were small compared with most previous meetings. An erroneous impression appears unfortunately to have gone abroad that Newport was a very costly place to visit and that the 'height of the season' at a great watering place was not a time when science could hold ground against fashion. While all this was eminently untrue, the effect of such an impression was visible in the absence of numbers of familiar faces.

Neither can we, if we would, conceal the fact that while many papers of marked ability were presented, the character of this meeting was not in all respects creditable to American Science. A conviction prevailed among many who were present at Newport of a decadence in the scientific character of the Association, of a loss of tone which if not already a demoralization threatened soon to become such. There was a time when weak speculations—by whomever put forth—were promptly trampled on and their authors made to feel that they were answerable to a power of united public sentiment which bore rule with a righteous severity. The evidence of this power was wanting at Newport—signs of concession to scientific charlatanism were visible in quarters where we least looked for it, and we left the meeting with a persuasion that unless the old order of proceedings could be restored, the American Association must come to a speedy and disastrous end. We do not despair of the immediate restoration of a higher standard—the effort will certainly be made.

One of the bright points of the Newport meeting was the opportune return of the Labrador Astronomical Expedition, sent out under the auspices of the United States Coast Survey and under the immediate sci-

* Furnished gratuitously to all members by the City Government.

entific leadership of Prof. Stephen Alexander of Princeton, to witness the total Eclipse of July 18th. As we elsewhere give a detailed statement of the results from the pen of one of the expedition, it is only necessary to add that the "*Bibb*" made Newport harbor on her return in season to secure an agreeable reunion on Tuesday evening, and that on Wednesday morning Prof. Alexander recounted the results of the Expedition to that land of 'monumental desolation' in a brief but fervid recital, deeply moving his audience by his peculiar eloquence. It was a happy circumstance that the meeting could close under so agreeable an impression.

The address of the retiring President, Prof. Stephen Alexander, was delivered on Wednesday morning, just before the adjournment, and will appear in the Transactions of the year.

Prof. Bache gave an interesting public discourse reviewing the history of discovery, and the method of investigation employed in regard to the *Gulf Stream.* This address we hope to present to our readers in full in a future number of this Journal, as also the substance of another public address by Prof. Henry, on Atmospheric Electricity, delivered as a popular evening lecture at Newport, by the distinguished Secretary of the Smithsonian Institution.

List of papers presented to the Association.

Section A—Mathematics, Physics and Chemistry.

General Account of the Results of Part II of the Discussion of the Declinometer Observations made at the Girard College, Philadelphia, between the years 1840 and 1845, with special reference to the Solar Diurnal Variation and its Annual Inequality; by A. D. Bache.

On some of the Relations of the Violet and Green Modifications of Chrome Alum to Soda, Potassa and Ammonia; by E. N. Horsford.

Reflections on the Origin, Development and Changes of Languages; by Henry M. Harman.

The Great Auroral Display of August 28 and September 2, 1859; by Elias Loomis.

Brief Abstract of a Memoir on the Theoretical Determination of the Dimensions of Donati's Comet; by W. A. Norton.

On the Solution of Ice in Inland Waters; by B. F. Harrison.

On Natural Ice-Houses and on Frozen Wells; by Elias Loomis.

Description of a new Registering Thermometer; by James Lewis, of Mohawk, N. Y. Presented by W. B. Rogers.

On the Phenomena presented by the "Silver Spring," in Marion County, Florida; by John LeConte.

Influence of difference in the mean velocity of winds from the different points of the compass, in modifying the mean direction of the atmospheric currents over the United States; by James H. Claflin.

Can the Sudden Cooling of one Part of a Metallic Rod cause another Part, as a consequence, suddenly to become warm? by E. N. Horsford.

Improvement in Barometers; by H. A. Clum.

General Account of the Results of Part III of the Discussion of the Declinometer Observations made at Girard College, Philadelphia, between the years 1840 and 1845, with special reference to the Investigation of the Influence of the Moon on the Magnetic Declination; by A. D. Bache.

Some experiments and inferences in regard to Binocular Vision; by W. B. Rogers.

On the data and methods of the Hindu Astronomy; by W. D. Whitney.

On Atmospherical Electricity; by Joseph Henry.

Note on Sources of Error in the employment of Picric Acid to detect the Presence of Potash; by M. C. Lea. Presented by B. Silliman, Jr.

2. *Reclamation by Gov. Stevens of Washington Territory.*—Prof. B. SILLIMAN, Jr.—*Sir* :—I send you herewith a copy of my final Report and Narrative of the Exploration of the Northern route for a Pacific Railroad, and desire respectfully to ask you to examine it in connection with a volume entitled Natural History of Washington Territory, by Drs. Cooper and Suckley, also sent with this.

It is due to one of the authors (Dr. Suckley) that I should here remark that when he was informed of the injustice done me, he frankly admitted that I had a right to be dissatisfied and that nearly all the facts complained of occurred during his absence in California—that when he returned he found the title pages and the chapter on Meteorology had been printed under the direction of Dr. Cooper, my friend and neighbor, and that he supposed, from Dr. Cooper's relationship to me, that my permission had been given to the incorporation of the chapter on Meteorology. He also added that as an *amende honorable* he should send notices over his signature to every holder of the book and also one to be published in your Journal, stating that my name was inadvertently left out of the title page and that I was the *sole* author of the chapter on Meteorology. Therefore wherever Dr. Suckley's name is referred to, it is simply because he appears as joint author of the "Natural History," and not because I attach any blame to him in regard to the matter.

An examination of my report will show you that this "Natural History" is a portion of my own Report. I will first call your attention to their letter addressed to me transmitting their volume. You will find it at the close of the volume (Appendix C.). In that letter, as will appear from its contents, they transmit the portion of my Report immediately following the alphabetical Index and preceding the appendices.

If you will examine the "Natural History" you will find that it contains Chapter IV. of my Report on Meteorology, copied page for page; that this chapter is illustrated by several views taken from the body of my Report, and that my isothermal chart is to be found at the end of the volume. By turning to my own Report, it will be observed that this chapter on Meteorology is an integral portion of my own personal and official Report of the route, follows the geographical memoir and precedes the estimate of the cost of a railroad on the northern route.

You will also observe upon page viii. of the preface to the "Natural History" that the authors (Drs. Cooper and Suckley) state that none of the plates illustrating their volume have been before published in any of the series. If you will compare these pictorial views with those in my final Report and narrative, you will find them to be identically the same, having been struck off from the same stone, by the same person, and therefore cannot be new.

My object in addressing you is to expose the plagiarism of Dr. Cooper and to show his injustice to myself.

It being doubtful whether extra copies would be ordered by Congress, I gave my consent to the Government printer's striking off some extra copies of the Natural History Report for the use of Drs. Cooper and Suckley, they bearing the expense, and the pretended edition of Baillière Brothers was struck off in this manner. I did not give my consent to the incorporation of the chapter on Meteorology, or to the use of the

plates, or to the use of the isothermal chart. It was done without authority, without the least consultation with me, nor was I aware till I saw the bound volume that they had any intention of the kind.

Now turn to their title page. 1. My name is altogether omitted from it, whereas since the work is simply a portion of my own report, my name should have been there. 2. The title states that Drs. Cooper and Suckley are the authors of the work, and of course the authors of Chapter IV. The following extract from the title makes this claim:

"Being those parts of the final Reports on the Survey of the Northern Pacific Railroad route, containing the Climate and Physical Geography, with full catalogues and descriptions, &c., by J. G. Cooper, M. D., and Dr. G. Suckley, U. S. A., Naturalists to the Expedition." 3. The title shows new matter added, which, under the arrangement referred to, was certainly inadmissable.

I will now call your attention to Baillière's advertisement which I send you with the "Natural History." It is there set forth that Chapter IV, on Meteorology, is by Gov. Stevens and Dr. Cooper.

By reference to the date of my letter to the Secretary of War, transmitting my Report, and the date of Drs. Cooper and Suckley's preface, it will appear that on the 7th of February, 1859, this chapter was sent in to the Secretary as my exclusive work and was so published by the Senate. In November following it is claimed by Drs. Cooper and Suckley in their title page as their work, and in Baillière's notice as the joint work of Gov. Stevens and Dr. Cooper. The priority of the Senate publication establishes the plagiarism.

The authorship of the chapter in question is exclusively my own. Dr. Cooper has no pretext to any portion of the authorship. He was employed by me under the authority of the Department, at the rate of $125 per month, to assist me in my Report. He did assist me on this chapter and in other portions of my Report. I prescribed the mode of investigation, supervised the work daily and prepared the Report. The mode of investigation, reasoning, deductions and conclusions are my own and not Dr. Cooper's. My attention had long been given to the subject. The Doctor was patient under my direction in preparing tables and collecting data. It is not only a most extraordinary case for an employé to assume the authorship of an article when he simply did the work of an employé, but it has been long since established judicially that all such claims are utterly untenable.

Why did not Dr. Cooper claim the authorship when I sent in the Report to the Secretary? Why did he not claim it when he was employed by me in assisting me in the tables to be found in it? He was during this whole time and for many months subsequently at my house every day. Why did he not claim the authorship when the Senate Report was published? It was published before the date (November) of their preface.

Why has it been done clandestinely, my first notice of it being seven months after the Senate publication?

I had a right to expect better treatment at the hands of Dr. Cooper than this, since at the time he was contemplating claiming authorship to an article of which he knew I was the author, he was pretending friend-

ship towards myself, and afterwards did seek and obtain through my influence a position with a Government expedition.

I have respectfully to request that you will publish this communication in your valuable Journal. I am, Sir, very respectfully,

ISAAC I. STEVENS.

3. *Dr. Suckley's Disclaimer.*—Editors Silliman's Journal:—Gentlemen:—You will confer a favor by stating in the next number of your valuable Journal that the authorship of the Chapter on Meteorology in Cooper and Suckley's work on the Natural History of Washington Territory should be accorded to the Hon. Isaac I. Stevens, whose name was unfortunately omitted from the title-page. Notices to this effect have been sent to all the owners of copies.

Very respectfully, your ob't servant, GEORGE SUCKLEY.

New York, July 31, 1860.

4. *Stereoscopic Advertisements;* by ELI W. BLAKE, JR.—Prof. H. W. Dove, to whom we are indebted for so many beautiful stereoscopic experiments, in his *Optische Studien*,* gives a specimen of stereoscopic printing to illustrate the double refraction of Iceland Spar, as seen in *binocular vision.*

This effect is produced by printing for the left eye, lines in the ordinary manner, while for the right eye, the alternate lines are slightly advanced. Upon combining the two by means of the stereoscope, the printing appears to be in two planes, more or less distant from each other.

Should the "spacing" of the two not correspond *exactly*, single words will rise towards or fall back from the eye; thus by varying the spaces between the words even in the slightest degree, a marked effect is produced. In this age of advertising it might be worth the while of enterprising individuals to print their advertisements in a stereoscopic form. It would certainly gain for them a more general and careful study than this class of literature can generally command. I annex, as an example of this mode of printing, a stereoscopic advertisement of this Journal, which may be observed by simply placing the instrument over it.

5. *Paraselena and Lunar Rainbow;* by Lieut. J. M. GILLISS.—When returning from Washington Territory and at 9 P.M. of July 26th, we were witnesses to atmospheric phenomena so rarely seen together that a brief notice is offered for record in the Journal.

The steamer's position was lat. 44° N.; long. 124° 30′ W., and, consequently, some 20 miles N.W. of Umpqua river, in California. The stars shone brightly overhead and the atmosphere was perfectly calm, but loaded with a wet fog or mist. At the time specified, the moon was 8½ days old and its altitude about 20°. Its disc was enveloped in bright mist, which extended in a well-defined circle of 30′ to 32′ diameter. This mist-ring was surrounded by an exquisitely marked halo of about 3° diameter, whose colors were blue, pearl-white and reddish-pink, the blue color nearest to the moon. The halo lasted from 15^m to 20^m, its colors gradually fading, until they were no longer distinguishable, though the bright inner mist-ring continued much longer. When it had disappeared stars were visible to within 15° of the horizon.

* Optische Studien von H. W. Dove. Berlin, 1859.

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Prior to the formation of the halo, and for some time after, we ceased to see it, there was a perfectly formed fog or mist bow to the north of the ship. At times, a slight tinge of dirty red could be traced on its outer border and near the sea, though the predominant color was a murky white.

At 9^h 30^m a faint secondary bow was formed of 3° lesser radius, but it continued only a few minutes.

6. *Oil Wells of Pennsylvania and Ohio.*—A strong impulse has been given to the explorations for petroleum by the success of the well at Titusville, Pennsylvania, worked for some years past by the Pennsylvania Rock Oil Company, on Oil Creek. In a circuit of five miles from Titusville, there were early in July, over 400 wells in progress, and 100 at work, raising from ten to fifty barrels daily from depths varying from 40 to 300 feet. The crude oil is dichromatic, having by transmitted light a dark brown color, which by reflected light is greenish or blueish. Its density is 0·882. It is even in warm weather rather thick, and in cold weather is more stiff, but even at −15° is still fluid. Its odor is strong and peculiar from the Pennsylvania wells—but from the wells at Mecca, Ohio, it is nearly odorless. It boils at a very high temperature, but begins to distill a thin colorless oil, even at 212° F. By fractional distillation, I obtained from 304 grams of crude oil of Titusville:—

	Temperature.	Quantity.	Density.
1st Product	at 100°C. = 212°F. (acid water,)	5 Gms.	
2d "	" 140°C. to 150°C.=284° to 302°F.	26 "	·733
3d "	" 150°C. to 160°C.=302° to 320°F.	29 "	·752
4th "	" 160°C. to 170°C.=320° to 338°F.	38 "	·766
5th "	" 170°C. to 180°C.=338° to 356°F.	17 "	·776
6th "	" 180°C. to 200°C.=356° to 392°F.	16 "	·800
7th "	" 200°C. to 220°C.=392° to 428°F.	17 "	·848
8th "	" 220°C. to 270°C.=428° to 518°F.	12 "	·854

The *boiling points* of these several fluids present some anomalies, but are usually progressive, thus,

No. 2.	common boiling at 115°C.=239°F.	remained constant at 228°C.=442°F.
3.	" " 120°C.=248°F.	" " 270°C.=518°F.
4.	" " 140°C.=284°F.	" " 290°C.=554°F.
5.	" " 160°C.=320°F.	and still rising at 308°C.=581°F.
6.	" " 135° C.=275°F.	rose rapidly above the range of mercurial thermometer.
7.	" " 135°C.=275°F.	and still rising at 305°C.=305°F.

The first five products remained entirely fluid at the low temperature of −15° F.

At the town of Mecca, Trumbull County, Ohio, many Oil Wells have lately been bored. We learn from Dr. J. S. Newberry, who has lately visited them, that only two were as yet considerably productive. One at 'Powers Corners,' which yields three or four barrels of crude oil, daily,—and the second has rewarded the industry of two poor Germans by the unexpected yield of twenty-five gallons hourly, or from twelve to sixteen barrels daily. Over fifty wells were already in progress. Many of them yielding oil from the sand near the surface, and all easily bored—often for a well to the depth of fifty feet at a cost not above fifty dollars—without the pumping apparatus and lining. Oil springs have been known in both

these districts for over fifty years, and were regarded by the agricultural population with disgust, as spoiling their water—until they found them an unexpected source of wealth. Both these districts are within the coal region—but the Pennsylvania locality, at least, is below the coal-bearing rocks.—s.

7. *Artesian Well at Columbus, Ohio,—rate of increase in temperature at* 2,575 *feet depth.*—In a letter to the Editors from T. E. Wormley, Esq. dated Columbus, Ohio, August 6th, 1860. Dear Sirs :—I herewith send you the temperature of the artesian well in our city, at a depth of 2.575 feet.

A few days since a Walferdin's Thermometer, placed in a glass tube filled with water, and this enclosed in a strong iron case, also filled with water, was lowered to a depth of 2,475 feet, where it remained for twenty-five hours, it was then sunk to the bottom of the well, a depth of 2,575 feet, where it remained for forty minutes. Upon the withdrawal of the instruments, it was found to have registered 88° F. Assuming this to be the temperature at the bottom of the well, and also assuming as correct data, that the temperature is uniformly 53° F. at a depth of 90 feet, we have an increase of 1° F. for every seventy-one feet.

8. *Salt Wells in Michigan.*—At *Saginaw*, Michigan, a well 669 feet deep has been sunk, passing through the coal measures and reaching about to the top of the Devonian. Its water is half saturated with salt and has the temperature at surface of 54° F.

At *Grand Rapids*, Kent County, Michigan, on the Grand River, the salt well is 661 feet deep—but as it begins 445 feet lower down in the section than the Saginaw well, the whole section presented by the two is over 1,100 feet. Careful sections of all the beds passed through have been preserved by Mr. George A. Lathrop, our informant, who will communicate them to the State Geologist.

It would be interesting to record the bottom temperature of all artesian wells, by a Walferdin's thermometer, and we trust our correspondents will take care to send us such results.

Book Notices—

1. *A Treatise on Elementary and Higher Algebra ;* by Theodore Strong, LL. D., Prof. of Math. and Nat. Philosophy in Rutgers College, etc. New York. Pratt, Oakley & Co. 1859.—This work does not belong to the class of elementary compilations with which our schools and colleges are so abundantly supplied, but is a fresh and original treatise, stamped on every page with the peculiar impress of the author's mind. It contains moreover several positive additions to the Science while many of the demonstrations of well known propositions are new and highly suggestive. The leading idea of the author appears to have been to set forth the theory of the resolution of numerical equations in a new manner ; and accordingly this subject is entered upon at as early a stage of the book as possible. Thus he begins its discussion under the head of *Multiplication*, continues it successively under the heads of *Division*, *Geometrical Progressions*, and *Undetermined Coefficients*, and finally completes it in two chapters, near the close of the volume, on the "Limits of the Real Roots of an Equation of the *n*th Degree,"—and the "Development of the Real and Imaginary Roots of an Equation of the *n*th Degree." This distribution of the subject, however, has not conduced to clearness, while it has given the work a character which renders the word

"Elementary," introduced into the title, quite inappropriate. We should not have thought it necessary to make this remark had not the author in his preface declared that the work was intended "to give a full view of the present state of the science," and also to be adapted "to the wants of teachers and pupils not only in our Colleges and Academies, but *in schools of a primary character*." Dr. Strong has risen to such heights in the science and has so long been an industrious explorer on its confines, that he has lost sight of the paths by which the novice must necessarily be introduced. But assuming the reader to have acquired a fair knowledge of algebra, including the general theory of equations, we would recommend him to read this work as a *review* in which the science is rearranged in a special order, and a new light is thereby thrown upon many of its processes. For Dr. Strong is a master of his subject and handles the most difficult parts of it with something of that kind of giant ease which we feel and admire so much in the great Euler.

From this statement, the reader will readily understand that a minute review of this work, pointing out all that is peculiar in methods, or in arrangement of methods, would be a profitless and superfluous labor. We will make but a single remark upon that part of the book which is prominently set forth as new and important both by the author and his reviewers—namely: the Solution of Cubic Equations "by pure algebra." The author's solution is certainly new, (at least new to us,) and extremely ingenious; but we can hardly concede that it is the only solution "by pure algebra," as appears to be claimed. For if the term "pure algebra" is used to distinguish the solution from that which is obtained by trigonometry, then we have other methods of solving the "irreducible case of Cardan's rule" by "pure algebra," quite as complete on this score as that of Dr. Strong. He obtains the roots only by a series of successive approximations, and does not present them in the form of a finite algebraic formula. Horner's method of solving equations appears to us to meet the case as well as Dr. Strong's, both being based upon successive approximation, and in practice Horner's method will generally be far more expeditious. Again, the roots may be developed in converging series (as was done by Nicole as long ago as 1738) and thus obtained by "pure algebra." As compared with the method by series, Dr. Strong's solution possesses the decided advantage that it *always* approximates with tolerable rapidity, the rate of approximation being nearly the same in every case; while the series of Nicole converge with more or less rapidity according to the relation existing between the coefficients of the given equation, and in certain cases the convergence is so slow as to render the series *practically* useless. For example, the terms of the series which express the roots of the equation $x^3-23x=30$, will depend upon the successive powers of the fraction $\frac{24300}{24368}$, which differs so little from units, that one thousand terms of the series would barely suffice to determine the first decimal place of the root, whereas some ten or twelve approximations according to Dr. Strong's method will determine the roots accurately to six decimal places. This method, therefore, may be justly claimed as an addition to the science, as well as the solution of Binomial Equations, the new demonstration of the Binomial Theorem, the development of the roots of equations, series, etc., all of which will be read with pleasure by advanced students.

2. *Contributions to the Palæontology of Iowa, being descriptions of new species of Crinoidea and other fossils,*—(supplement to vol. i, part ii, of the Geological Repository of Iowa); by JAMES HALL.—Eight sheets of an early copy of these contributions have reached us from the Author in advance of publication. As we hope soon to receive a complete copy, we reserve a list of its contents for a second notice.

PERSONAL.—Prof. DANA returned to the United States early in August, improved in health by his European visit, but not yet in a state to permit the resumption of his accustomed labors. LADY FRANKLIN was passenger on the same ship.

Prof. C. U. SHEPARD, and Prof. ALEXIS CASWELL of Brown University are among the American tourists in Europe this season.

Prof. ELIAS LOOMIS, LL.D., late of New York University, has been elected to the chair of Natural Philosophy and Astronomy in Yale College made vacant by the death of Prof. D. Olmsted. Prof. Loomis is now in Europe for the purpose of making additions to the physical cabinet of Yale College.

JOSEPH E. SHEFFIELD, Esq., a citizen of New Haven, distinguished for his enlightened liberality, has at his sole charge, prepared a large and commodious building for the uses of the YALE SCIENTIFIC SCHOOL, embracing ample apartments for the Engineering, Mechanical and Physical departments—a perfectly appointed chemical Laboratory fitted for thirty special students, and private laboratories for the Professors—a Metallurgical Laboratory—Lecture rooms, both private and public—and two museum rooms—forming altogether a scientific Establishment, second to none in this country. It will be ready for occupation at the opening of the fall term.

TRANSACTIONS OF THE AM. PHILOS. SOCIETY, Phila., vol. xi, new series. Part III, 1860.—Art. xvi, p. 187–258, Revision of Buprestidæ of the U. S., with a plate. By *John L. LeConte, M.D.*—xvii, p. 259–402, Analytic Orthography; an Investigation of the sources of the voice and their Alphabetic notation. By *Prof. S. S. Haldemann.*

PROCEEDINGS OF AM. PHIL. SOC. PHILAD., 1860, January to June.—p. 174, Deaths of members announced—p. 175, Phosphoresence of the Diamond; *Dr. Emerson.*—p. 176, Gale of July 9th and 10th, 1860—Effects upon Philadelphia Gas Works; *Prof. Cresson.*—p. 177, Sanscrit and English analogues; *Pliny E. Chase.*—Geology of the Arctic Archipelago drawn from Mc Klintock's narrative, *J. P. Lesley.*—p. 295, Registering Thermometer; *James Lewis.*—p. 297. Biographical notice of the late THOMAS NUTTALL—p. 320. Optical education: *Dr. Emerson.*—Obituary notice of JOSEPH ADDISON ALEXANDER; *John Leyburn, D.D.*

TRANSACTIONS OF THE ACADEMY OF SAINT LOUIS, (Mo.;) vol. i.—p. 583, Observations on the cretaceous strata of Texas; *B. F. Shumard.*—p. 606, Descriptions of new cretaceous fossils from Texas; *B. F. Shumard.* The author describes the following: *Nautilus Texanus; Janira Wrightii; Ostrea quadriplicata; O. bellaplicata; Cidaris hemigranosus*—p. 624, Description of five new species of Gasteropoda, from the Coal measures, and a Brachiopod from the Potsdam Sandstone of Texas, *B. F. Shumard,* viz: *Pleurotomaria Brazoensis; P. tenuistriata; P. Riddellii; P. glandula; Murchisona Texana; Orthis Coloradoensis.*

MÉMOIRES DE L' ACADÉMIE IMPÉRIALE DES SCIENCES, ARTS ET BELLES-LETTRES DE DIJON, second series, Tome vii, 1858–1859. Dijon, 1859, 800 pp., scientific contents *catalogues des Insectes coléoptéres du département de la Cote-d'or (suite), par M. Rouget.*

Une visite á la grotte de Fouvent (Haute-Saône): Ossements fossiles et debris de l'industrie humain; memoire posthume de *M. Nodot.*

POSTSCRIPT, September 7.

Le Verrier's Report on the Solar Eclipse of July 18, 1860, *at Tarazona in Spain*, (*L'Institut*, Nos. 1387–88–89. Aug. 1–16.)—[At the last moment, and after our notices of this phenomenon were printed (see pp. 281, 285, 288), we have received LeVerrier's Report of the Observations of the French Expedition to Spain, made to the Minister of Public Instruction, which we hasten to lay before our readers, slightly condensed, although other matters which some of our correspondents will naturally look for here are thereby displaced. The interest with which LeVerrier's new views of the physical constitution of the Sun will be read is our apology to all such.]

LeVerrier was accompanied to Spain by Messrs. Yvon Villarceau, and Chacornac, who were occupied chiefly with determining the height and position of two or more of the luminous appendages. M. Foucault studied the corona, and made the photometric and photographic experiments. M. LeVerrier observed the astronomical phases of the phenomenon, and was also charged with the duty of obtaining an exact description of the whole scene. Two telescopes on Foucault's plan were devoted to the measurements, being provided with micrometers of peculiar construction, devised by Yvon Villarceau for rapid and easy manipulations in the dark. Two excellent telescopes of 6 inch aperture (one for the use of the Spanish observers) were also provided, to which must be added the photographic apparatus, a meridian circle, chronometers, barometers, seekers, and lastly the great meridian instrument belonging to the War Department, and with which the longitude was determined—forming a grand total of scientific baggage which on the 28th of June was dispatched for Spain. The outfit of the English expedition was even yet more considerable. Early in July Mr. Yvon Villarceau joined the instruments at Tudela in the center of Spain, on the banks of the Ebro, and immediately proceeded with them to Tarazona and to the chosen station called the *Sanctuaire*, 1,400 metres (=4592 feet) above the sea. M. LeVerrier and Foucault, fearing clouds, descended on the morning of the 18th to a plateau near the cemetery of Tarazona where the weather was magnificent during the whole eclipse. Passing the description of the contacts and observations for time, &c., we note that at totality they found the general illumination of the atmosphere much greater than the relation of former observers of total eclipses had led them to expect, so that they could read and write easily without using their lamps.

Says LeV.: "The first object which I saw in the field of the telescope after the commencement of totality was an isolated cloud separated from the moon's border by a space equal to its own breadth, the whole about a minute and a half high by double that length. Its color was a beautiful rose mixed with shades of violet, and its transparency seemed to increase even to brilliant white in some parts. A little below on the right two clouds lay superimposed on each other, the smaller above, and the two of very unequal brilliancy. The rest of the western edge of the disc and the lower part showed nothing more than the corona, the light of which was perfectly white and of the greatest brilliancy. But 30° below the horizontal diameter on the east I discovered two lofty and adjoining

peaks, the upper sides both tinted with rosy and violet light while the lower sides were brilliant white. I do not doubt that the toothed form I assign to these peaks is real, which as it contrasted with that of the first appendages I have described, I verified with great care; moreover, in shifting the telescope, whose high power permitted a sight of only a small part of the solar disc at one time, I saw a third peak a little higher, also tooth-formed, and resembling the two others in color and form, differing only in its larger dimensions. The remainder of the disc offered nothing remarkable, and on returning to the upper region I found the two first described clouds unchanged. As the moment of reappearance of the Sun approached, and while waiting for the first rays, I made, during about 20^s, perhaps my most important observation. The margin of the disc which two minutes before was entirely white was now tinged by a delicate fillet of unappreciable thickness of a purplish red—then as the seconds glided by, this fillet enlarged by degrees and formed soon around the black disc of the moon, over a breadth of about 30° a red border perfectly defined in thickness, crescent-formed and with an irregular outline above. At the same instant the brilliancy of the part of the corona which during the last second emerged from behind the moon's disc was exalted so rapidly that I was in doubt if the sun's light was not returned. It was only on the reappearance of the direct rays, the brightness of which obliterated in turn the corona, that I was sure of the nature of three phenomena present at the same time, which I thus sum up.

1. The visible part of the emergent Sun over its whole breadth and up to the height of seven or eight seconds was covered by a bed of rosy clouds, which appeared to gain in thickness as they emerged from behind the disc of the moon. Must we believe that the entire surface of the sun is overspread at a small elevation as it is strewn with faculæ, and that the roseate clouds are emanations, appearing like spots on the sun's disc?

2. The intensity of the light in the corona which is always white, varies with great rapidity in the immediate vicinity of the Sun's disc.

3. The reappearance of the direct sunlight was at $3^h\ 0^m\ 49^s{\cdot}0$. Total obscuration continued $3^m\ 14^s{\cdot}3$. The disc of the moon completely cleared the Sun at $4^h\ 6^m\ 20^s$."

[M. Foucault's interesting observations on the photographs, etc., are unavoidably postponed for want of room.]

LeVerrier goes on to state that the observation of his party authorize, in his opinion, important modifications in the generally received notions respecting the physical constitution of the Sun. Arago in his notice of solar eclipses—says, "where exist the reddish flames with well defined outlines which during the total eclipse of the 8th of July, 1842, passed considerably beyond the outlines of the lunar disc? These flames were either in the Moon, or in the Sun, or in our atmosphere; unless, indeed, denying their actual existence, we regard them as an effect of light, for example as phenomena of diffraction."

The two last suppositions have found few partisans. Before adopting any hypothesis it is necessary to decide by observation a certain feature of the phenomenon. During the eclipse, the disc of the moon moves across the disc of the Sun. But do these reddish clouds follow the moon in

its movement? or does each cloud remain invariably above the same point on the solar disc? In the first case the origin of the luminous clouds is to be sought in the Moon; in the second case, these clouds belong to the Sun. For clearness sake, assume the latter supposition, and observe what appearances should present themselves when the lunar disc passes like a screen over the whole. Consider first a cloud situated on the east and adherent to the Sun's limb. This object will be visible at the instant when totality commences. The advancing moon will regularly, at the rate of a half second of arc in a second of time, cover with its limb successively the lower then the middle and lastly the higher portions—thus constantlv diminishing the height of the cloud. For a cloud situated on the west, these appearances will be reversed, its magnitude increasing as the moon gradually uncovers it. If then the roseate appendages seen during a total eclipse, depend on the sun, the fact should appear by the variation in height between those which appear in the east and the west. The phenomena will appear otherwise if the clouds appertain to the moon.

In the absence of equatorial solar clouds, the question in dispute can still be decided by observations on those seen on the south or north of the disc. The height of these clouds ought not to vary, it is true, whether they belong to the moon, or to the sun, but in the latter case, carried away by the sun they would be displaced on the lunar disc with a certain velocity, while if they are adherent to the moon's disc, they would not be so displaced Hence the study of the height of the luminous clouds, whether east and west or north and south, has the highest interest. All the elements of the desired demonstration are found in the Spanish observations. In my first report, I mentioned the successive increase in thickness of a band of rosy clouds visible from the east to the end of the eclipse. Messrs. Yvon Villarceau and Chacornac have carefully noted the motions of a cloud, situated on the north. This cloud, according to M. Villarceau, in two minutes' time was displaced $3\frac{1}{2}°$ on the moon's disc, in moving to the west. The measurements of M. Chacornac cover an interval of over six minutes in time, in which the cloud moved $11\frac{1}{2}°$.

Beautiful observation, and one which could not have been hoped for! We see that the duration of the motion studied by Chacornac, much exceeds the time of total obscuration. The last measure was made more than three minutes after sun-light had reappeared! It is important to note, among other points that this was not done with a cloud vaguely seen after the return of sunlight, but fortunately it was a measurement so carefully made as to be a guaranty against the possibility of illusion. It should be added that the displacement of the luminous cloud determined by the observations made at the *Sanctuaire*, is precisely equal to that required by calculation, assuming the cloud to belong to the Sun. There remains, then, no foundation for a doubt, as to the nature of the rosy clouds which have been variously called flames, mountains, protuberances, and clouds.

The observation on one of the appendages, perfectly isolated from the disc of both the Sun and Moon and of a sharply pronounced character, and on the other the appearance of a rosy band on the west at the moment of emersion, and the rate of motion of a second appendage, fixed by Villarceau and Chacornac, prove that these objects belong to the Sun.

Let us then hereafter give the name of *solar clouds* to the rosy appendages which become visible when the solar light is sufficiently dimmed.

A few words more will finish the description of the phenomenon and of the observations. Ismaïl-Effendi, a young Egyptian attached to the Paris Observatory for three years past, a very expert astronomer, and who accompanied the French expedition to Spain, has sent me a drawing which proves the appearance of luminous clouds in the east immediately before the commencement of the eclipse. The clouds in question form a slightly elevated but continuous band embracing 90° of the outline of the Sun. This band was not long visible, but was eclipsed behind the lunar disc, and it had in effect ceased when I passed over this region in exploring the whole periphery with a power which allowed me to see only portions successively.

The magnetic observations were made at Paris, the variations being sensibly simultaneous for the whole of Europe, and M. Desains, who took note of the magnetic observations, detected no perturbations during the eclipse.

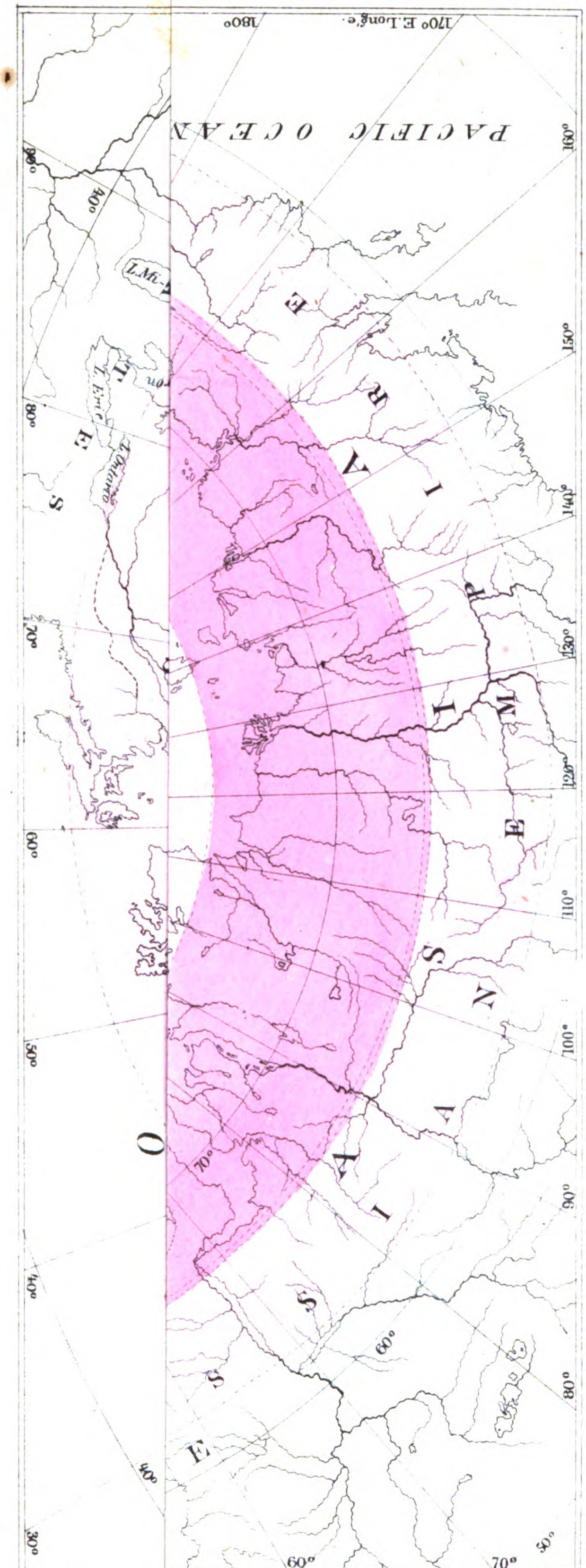
Physical constitution of the Sun.—A reconstruction or even a complete abandonment of the theory hitherto prevalent as to the physical constitution of the Sun appears to me essential. It must give place to one far more simple.

We have been hitherto assured that the Sun was composed of a central dark globe; that above this globe existed an immense atmosphere of sombre clouds, still higher was placed the photosphere, a self-luminous, gaseous envelope, and the source of the light and heat of the sun. Where the clouds of the photosphere are rent, says the old theory, the dark body of the Sun is seen in the spots which so frequently appear. To this complex constitution must be added a third envelope formed of the accumulation of roseate clouds.

Now, I fear that the greater part of these envelopes are only fictions—that the Sun is a body, luminous, simply because of its high temperature, and covered by an unbroken layer of roseate matter whose existence is now proved. This luminary thus formed of a central nucleus, liquid or solid, and covered by an atmosphere, falls within the law common to the constitution of celestial bodies.

[M. LeVerrier goes on to discuss with some detail the solar spots in the light of these new views, but this we must defer for another occasion. It is certain that a subject of so much interest will command much consideration from physicists and astronomers and we shall take care to give it the attention it deserves.

Nor will the question be settled peaceably—already M. Faye (*Comptes Rend.*, Aug. 13) in presenting to the French Academy a long letter from Baron Feilitzsch with an account of his observations (also in Spain), declares it to be his opinion as well as that of Baron F. that the eclipse of 1860 furnishes the most decisive evidence in favor of the opinion which refers the corona and the luminous clouds to simple optical appearances, and not to the essential constitution of the Sun or of his atmosphere. M. Faye adds that the opinion appears to be confirmed by a comparison of the results of other observers—that the Sun has no atmosphere and that the appearances recorded are purely optical!—Eds.]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THE

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[SECOND SERIES.]

ART. XXVIII.—*Lecture on the Gulf Stream, Prepared at the Request of the American Association for the Advancement of Science;* by A. D. BACHE, Supt. U. S. Coast Survey.*

[Delivered before the American Association for the Advancement of Science, at Newport.]

BY request of the Association, at their last meeting, at Springfield, I now present a summary of the results of the Gulf Stream explorations made by the officers of the Coast Survey.

The Gulf Stream is the great hydrographic feature of the United States coast, and no survey of the coast could be complete for purposes of navigation, without it. Hence the explorations have been early undertaken and thoroughly carried on. But as it required peculiar means and special adaptation in the officers to this line of research, and did not require a continuous survey, the work has been executed from time to time, as means and officers could be had without interference with the more regular operations of the hydrography.

An act of Congress which refers to this Survey, requires the immediate presentation of its results to Congress and they have therefore been discussed as soon as procured and have been given to the public.

This is the great sea mark of the coast of the United States, both Gulf and Atlantic, and its qualities as hindrances and aids to navigation require that the navigator should be well informed in regard to it.

* Communicated by the Author.

In order to present an intelligible summary of the results obtained by the Coast Survey in the short time allowed for a lecture, it is necessary to condense the subject very considerably, to omit matters at all extraneous to the subjects in hand, and to confine myself to a brief and direct statement of *the means employed in examining the stream from its surface to its depths, the method of studying the results*, and *of the results themselves.*

The temperatures in and near the Gulf Stream, are among its most striking peculiarities, and therefore have formed one principal object of observation. It will be necessary in order to bring the subject within limits, to confine myself chiefly at this time to the consideration of this class of facts and to the results and laws connected with them.

I shall proceed therefore to consider the subject under the following heads:

1. The Instruments for determining depths and temperatures and for obtaining specimens of the bottom.
2. The plan of research.
3. The method of discussion of the results.
4. The results, consisting of type-curves of the law of change of temperature with depth, at several characteristic positions. Type-curves showing the distribution of temperatures across the stream, represented by two sets of curves, one in which the variable temperatures at the same depth is shown, and the other in which the variable depth of the same temperature is represented. Upon the diagrams showing these latter curves, the figure of the bottom of the sea is given, where it has been obtained.

Discussion in regard to the cold wall, which is one of the most interesting features of the approach to the Gulf Stream.

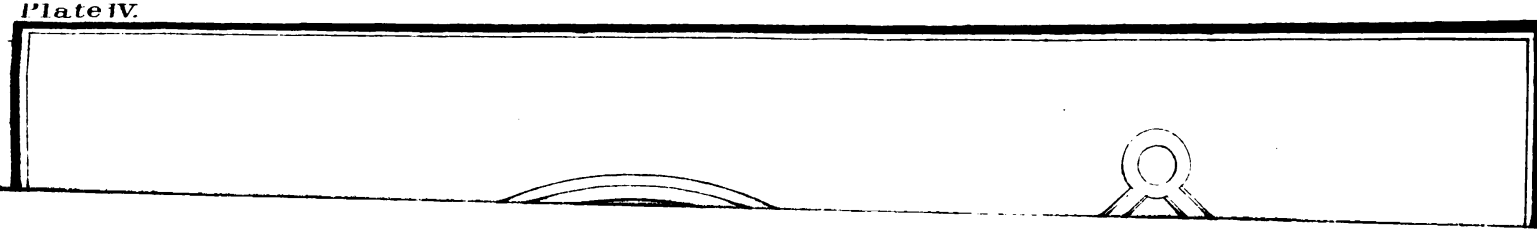
5. The limit of accuracy of the results.
6. The figure of the bottom of the ocean below the Gulf Stream.
7. The general features of the Gulf Stream as to temperature.

These points are illustrated by diagrams, enabling the eye to follow the results as they are stated.

I. Instruments.

1. *For Temperatures.*—The instrument for determining temperatures should fulfill the two conditions of registering its indications and of being unaffected by pressure. The common mercurial thermometer, while it answers perfectly for the determination of temperatures at the surface, fails in both the conditions stated. The ordinary self-registering thermometer, or self-registering metallic thermometer, in the watch form, as made by Breguet, Montandon, and Jürgensen, when provided with a suitable cover to protect it from pressure, answers a good purpose, and has been extensively applied in the course of the observations. As

Plate IV.



a rule it is only the minimum temperature thermometers that must be used, as the temperatures decrease generally in descending. An ordinary self-registering minimum thermometer placed in a glass globe, was successfully used by Commander Charles H. Davis, and by Lieut. G. M. Bache. It has the disadvantage of taking the temperature slowly, and of being inapplicable below a certain depth. Small hollow cylindrical brass vessels which were divided in two parts closely fitted by grinding, and within which the Breguet thermometers of the watch form were placed, were an improvement upon the glass globe, as taking the temperature of the sea more rapidly, but besides the difficulty of making the joint tight, they were crushed by the pressure, at even moderate depths. The substitution of a globe, for the cylinder, extended the range of these instruments, but the thermometers were often crushed or injured by the access of sea-water to the interior of the globe. Six's self-registering thermometers as bearing considerable pressure without injury and without rendering the indications erroneous, and as requiring no case to enclose them, except to prevent breaking from accidental knocks in handling, are very useful. They are still favorites with many of the officers, though others complain of their great liability to derangement, especially if the mercury is not perfectly clean, when the mercurial column easily separates and some skill is required to bring it together. These instruments are from their cheapness still furnished to the parties and are used successfully at depths reaching about one hundred fathoms, and on occasions, considerably lower. Keeping them in order requires the skill of an experimenter, rather than that of an observer, and hence they do not satisfactorily fulfill the conditions of the problem. The metallic thermometer of Joseph Saxton, Esq., of the U. S. Office of Weights and Measures, is a compound coil resembling somewhat the well known instrument of Breguet. In its construction, two stout ribbons, of silver and platinum—carefully united by silver solder to an intermediate thin plate of gold—are coiled with the more expansible metal in the interior. The gold serves to prevent the tendency of the silver and platinum to separate. The lower end of this coil is fastened to a brass stem passing through the axis of the coil, while its upper end is firmly attached to the base of a short cylinder. The whole motion of the coil as it winds and unwinds with variations of temperature, thus acts to rotate the axial stem. This motion is magnified by multiplying wheels contained in the short cylinder at top, and is registered upon the dial of the instrument by an index, which pushes before it a registering hand, moving with sufficient friction merely to retain its place when thrust forward by the index hand of the thermometer. These instruments are

graduated by trial. The brass and silver portions receive a thick coating of gold by the electrotype process to prevent the action of the sea-water upon them.

When kept clean by frequent washing in fresh water, and in good order and frequently compared with the standards to guard against accidental derangements, these thermometers answer admirably all the required conditions. The length of the coil measured along its axis should not be less than six inches, as the interposition of wheels to magnify the motion, should as far as possible, be avoided. The water being all around the coil, which is a good conductor, and has a low specific heat, the instrument readily feels the temperature of the part of the sea where it is exposed, and registers it to less than half a degree (say 0·2) with certainty. The box which covers the coil and indicating part of the thermometer is merely to protect it from accidental injury, and is open so as to permit the sea-water to pass freely through it. Plate IV gives a view of Saxton's metallic thermometer, and of its various parts in detail. Although there seemed no reason to doubt that this instrument was free from any effects of pressure, it was deemed desirable to actually try it by extreme pressure and a series of experiments made by J. M. Batchelder, Esq., showed that at pressures less than that corresponding to 600 fathoms, the effect was less than one degree (0°·25 to 1°) and at pressures from 600 to 1500 fathoms the change amounted to little more than from 7° to 9° Fahr., the index returning when the pressure was removed. For great depths the effects of pressure must be ascertained, as it is specific in each instrument and probably depends chiefly upon some mechanical defect in the construction, perhaps in the soldering.*

The apparatus used in these experiments on the effect of pressure, was a very ingenious one for testing hydraulic engines by Mr. Thomas Davison of the Novelty Iron Works of New York. Fig. No. 12, Plate IV.

2. *For Depths.*—Where the depth becomes considerable the usual sounding line fails entirely to give it, especially if there is a current and more especially if there is besides, a counter-current. The amount of "stray line" is very variable. This subject has been ably examined of late years by Commanders Maury and S. P. Lee, Lieuts. Berryman, Brooke and others of our navy, and by Commander Dayman and others of the British navy, and especially by Prof. Trowbridge of the Coast Survey in his memoir read before the Association ("Deep Sea Soundings," by W. P. Trowbridge, Assistant U. S. Coast Survey,) at the meeting in Baltimore and re-published in the American Journal of Science and Arts, vol. xxviii for the year 1858.

* Gulf Stream Explorations; Third Memoir Proceedings Amer. Assoc. Adv. Sc., 13th Meeting, Springfield, 1859, and this Jour., [2], vol. xxix, 1860.

The use of Ogden's or Ericcson's leads to 100 fathoms is still continued by some of the officers of the survey, though, at such depths, nothing better than the common sounding line is in fact required. Massey's lead with Woltman's wheel, as an indicator. has been extensively used of late years. Mr. Saxton's indicator is more simple than Massey's, but acts upon the same principle. To remedy the defect of the turning of the cord of the lead line, two indicators are applied, one on each side of the axis. Prof. Trowbridge's lead modified somewhat from that described at the last meeting of the Association in Baltimore, has recently been tried with good success by Lieut. Comdg. Wilkinson in the last soundings across the Straits of Florida for the telegraph to Havana. The most reliable observations heretofore made in the Coast Survey have been with Massey's indicator, the errors are not such as to affect the development of the laws of change at the moderate depth reached in most of the observations, and at great depths the changes are very slow. The new apparatus has the advantage of saving a great deal of time and therefore inaccuracies from change of position during the sounding are avoided.

3. *For obtaining specimens from the bottom.*—The only satisfactory test of having reached the bottom of the sea at considerable depths being the bringing up of a specimen, this has been a subject of constant study with us. The different instruments invented by Lieut. Stellwagen, Commander Sands, Lieut. Craven, Lieut. Berryman, Lieut. Brooke and other officers of our navy, are all in use for different kinds of bottom, and according to the preference given by different hydrographic chiefs. The one most commonly used in these explorations has been Lieut. Stellwagen's invention; a cup placed below the sounding lead, covered by a dick or valve of leather which slides up the stem of the cup and opens when the lead is descending, closing when it is raised. The weight of the lead and the turning of the cord generally suffice to sink the cup into the bottom, filling it, and when the valve is made to close tightly by a piece of flexible leather below the stiff disk, the specimen is not washed out as the lead is drawn up. In Commander Sands' sounding apparatus a spring keeps an outer cylinder over an opening in an inner hollow one, until it reaches the bottom, when the outer cylinder is forced upwards and the opening at the side of the inner one, which, having a conical termination, penetrates the bottom, permitting a specimen of the bottom to pass in. On raising the lead the spring forces the outer cylinder over the opening, preventing the specimen from being washed out. The only very deep soundings being, as a general rule, in soft bottom, Sands' specimen-cylinder is admirably adapted to that class of work.

II. Plan of the Work.

The plan of the work was simple. The temperatures were to be ascertained at various depths, at different distances from the coast, on sections as nearly at right angles with the stream as practicable, the sections starting from some point well known in position. The temperatures were to be taken at distances diminishing as the changes of temperature were more rapid. So in regard to the depths, the observations were to be multiplied in the strata of rapidly varying temperatures near the surface. So in regard to position, when the cold water near the coast was rapidly exchanging for the warm water of the Gulf Stream, the sections diminishing in distance as the source of the warm water was approached.

The vessel's position was determined with reference to some prominent point, Sandy Hook, or Cape May, for example, the course run was perpendicular to the supposed axis of the stream, S.E., several positions were taken up in succession and at each the temperatures ascertained at the surface, at 5, 10, 15, 20, 30, 50, 100, 200, 300, 400, 600 fathoms, or depths found to apply more satisfactorily under the general rule, to the position and section. Having crossed the stream any position found to be desirable, could be assumed on returning, and the extreme position reached was verified by the return to the coast.

The summer season was selected for the standard observations for various reasons, but chiefly for two, namely, that the weather permitted more accurate work, and the phenomena were more likely to be those of equilibrium, when the surface water was more slowly changing its temperature. Our little vessels could not, without considerable danger, be exposed to the roughness of the wind and water in the Gulf Stream in winter, and when we attempted comparative winter observations, disappointment was often the result. The loss of one valuable officer and ten of his crew, and the extreme peril of another in autumnal explorations of the stream, has but too fully justified these precautions. The propriety of selecting the summer for making the observations was completely proved by the success in determining the laws of temperature.

These observations were but incidental to the hydrography of the coast, and hence were prosecuted only when means could be spared from other more pressing and regular parts of the work. It was only a favorable conjuncture with regard to officers, means, weather, adaptation of vessel, and the like, which gave results even when attempted. Too much credit cannot be assigned to those who have succeeded in this laborious and perilous work, and their names have been kept in close connection with their results, whenever and wherever brought before the public, and they have been carefully preserved in the archives

of the Survey. Charles H. Davis, George M. Bache, S. P. Lee, Richard Bache, John N. Maffitt, T. A. Craven, Otway H. Berryman, B. F. Sands and John Wilkinson make up the list of our successful observers in this field within the last sixteen years. Their names you will see attached to the sections run by them on the general chart of the Gulf Stream presented to you this evening.

The first was run in 1844, from Nantucket south and eastward, by Commander C. H. Davis, now the accomplished Superintendent of the Nautical Almanac, and the last in 1860 by Lieut. John Wilkinson, from the Tortugas, southeast to the coast of Cuba. The work still goes on perseveringly.

The number of sections run has been fourteen, the number of positions on these sections occupied 300, and the number of observations made for temperature 3600. The limits below which the stream and the adjacent waters have been explored for temperatures are from latitude 23° N., to 41° N., and from longitude 83° W. to 66½° W. from near Havana to near Cape Cod, and from the Tortugas to 9½° E. of Cape Henlopen. The distance along the axis of the Gulf Stream to the most northeastern point in the North Atlantic, measures nearly 1400 nautical miles.

III. Method of Discussion of the Results.

These have generally been discussed by diagrams, sometimes by analytical formulæ; the former method is generally best adapted to the character and degree of accuracy and circumstances of the observation; the diagrams finally adopted after trials were chiefly of three different kinds, one for the discussion of the change of temperature with depths, the two others for the change of temperature with position as well as depth. Of the first of these diagrams Nos. 1 and 2, Plate I, are specimens. The depths constitute the ordinates and the temperatures the abscissæ of a curve, showing the law of change of temperature with the depth. Upon the horizontal lines at the top of the paper the temperatures from ten degrees to ten degrees Fahr. are written and on the vertical line at the side, the depths. The separate observations being represented by dots; the curve is drawn with a free hand among them.

The next two classes of diagrams give the distribution of temperatures across the sections. In the first the temperature corresponding to the same depth; in the second the depths corresponding to the same temperatures. In this latter the figure of the bottom is shown when ascertained. In both classes the distances from the cape, or headland, city or inlet, which is the origin of the section is marked, and the several positions occupied for observing, so that the abscissæ of the curve are the distances from

the point of beginning. In the first (see diagram No. 4, Plate I,) the temperatures are marked on the vertical lines at the left side of the diagrams, the ordinates of the curves thus corresponding to temperatures. In the second (see diagram No. 9, Plate I,) the depths are similarly written, the ordinates thus corresponding to depths. The notes or legend, show in the first case to what depths the curves correspond and in the second to what temperatures. The observations at each position being plotted according to its temperature or depth in the two classes of diagrams, the curve is drawn with a free hand among the points.

It should be observed that the discussion of each season's observations was in general made separately, and that the result of one, two or three seasons, grouped, were announced separately, leaving to the new observations to confirm, or refute, the conclusions drawn. It is a remarkable fact that with such difficulties in the way, in the character of the phenomena to be observed, in the diversity of seasons and of observers, the phenomena have always been readily deducible from the observations, and that the separate discussions have been confirmations, the following of the preceding; in short that the nature of the medium in which the work has been performed in its relations to heat, has more than compensated for other difficulties and that the results are more accordant than the elaborate ones obtained from the progress of temperature below the surface of the ground by the experienced and skillful observers who have made them. Few observations have been rejected in the whole series.

I need not notice special diagrams which will be explained when your attention is called to them.

When the character of the diagrams to be made had been definitely fixed, they were prepared under the direction of the chiefs of the parties, so that I was relieved of the personal labor of representing the results. In the subsequent general discussion I was greatly assisted by Prof. Pendleton, U. S. N. and by Prof. W. P. Trowbridge, Assistant U. S. Coast Survey, who has made a general review of the whole of the results preparatory to their publication in a volume of the Records and Results of the Coast Survey.

IV. Results.

1. *Type-curves of law of temperature with depths at the most characteristic positions.*—The two most characteristic positions are in the cold current between the land and the Gulf Stream and in the axis of the stream itself.

1. Diagram No. 1, Plate I, is a specimen of the type-curve in the cold current. The long tongue from the surface to about 50 fathoms in depth is the overflow of the warm water of the Gulf Stream, the temperature varying from 81° to about 55°. The

temperatures in the mass of water from 50 fathoms down to 500 fathoms are just such as would take place in a mass of water heated by conduction from the surface, the law is that of a logarithmic curve, in which the conducting power of sea water is the modulus of the system.

A comparison of many of these curves with the logarithmic form showed that it was applicable to them within the limits of the probable error of the observations. Taking the warm stratum from the Gulf of Mexico above and the cold polar stratum below, the mass of the water between is heated by conduction. The bottom of the sea has not been reached under the axis of the Gulf Stream, north of Cape Lookout on the North Carolina coast.

This form of curve was deduced in 1844 from the observations of Commander Charles H. Davis and was the first discovery made in connection with the then recently commenced systematic exploration of the Gulf Stream by the Coast Survey.

2. Nos. 2, 3, and 3 bis, Plate I, are specimens of the type-curve in the Gulf Stream, taken from the sections off Cape Henry, Cape Hatteras and Charleston, being characterized by the comparatively short beak or projection and the persistence of the higher temperature to great depths as 55° to 425, 450, 550 fathoms giving the peculiar shape to this curve between 50 and 500 fathoms.

II. Type-curves of distribution of Temperature across the Stream.

(*a.*) *Curves of temperature at the same depths.*—The sections made are the following, beginning the enumeration at the Gulf of Mexico: 1. Tortugas to Havana. 2. Sombrero Key to Salt Key. 3. Carysfort, L. H., to Cuba. 4. Cape Florida to Bemini. 5. Off Cape Cañaveral. 6. Off St. Augustine. 7. Off St. Simon's, Georgia. 8. Off Charleston. 9. Off Cape Fear. 10. Off Cape Hatteras. 11. Off Cape Henry. 12. Off Cape May. 13. Off Sandy Hook. 14. Off Cape Cod, being on the average one to each hundred miles along the axis of the stream. These are marked on the general chart, Plate III, the names of the explorers being stated in the column which gives the point of origin of each section.

The Sandy Hook curves, Nos. 4 and 5 Plate I, are among the best of the type-curves of temperature at the same depth, though among the earliest determined. The overflow of the Gulf Stream into the long space occupied by the cold current between it and the shore, mixing in a degree with the cold water, is well shown by the curves *a*, *b* and *c* at the surface, 5 and 10 fathoms, and the still greater admixture with the cold water at 20, 30 and 50 fathoms (*d*, *e*, *f*.) The whole space from the shore to 240 miles, is occupied, however, with comparatively cold water. Then is

met the sudden rise to the Gulf Stream shown especially below 50 fathoms and termed so appropriately by Lieut. George M. Bache the "cold wall," that navigators have not hesitated to receive the term into use; next the hot water of the Gulf Stream, rising to a maximum of 82°, then falling to a minimum of 80°, rising to a second maximum of 81½°, falling to a second minimum of 78° and rising from this toward a third maximum. With these results the curves at 5 and 10 fathoms and those at 20, 30, 50, 70, 100 and 150 fathoms agree and, with characteristic differences, those of 200, 300, 400 and 500 fathoms.

The cold wall at 20 fathoms shows a rise of 19° in 25 miles, three quarters of a degree to a mile, and at 200 fathoms of 16°, in the same distance; at the surface it is nearly 8° in 50 miles. The cold water between the Gulf Stream and the shore has two well marked maxima and two minima in it, of which one seems to correspond in position to the sudden deepening of the water 100 miles from Sandy Hook, as shown by the Coast Survey offshore chart between Gay Head and Cape Henlopen.

These results are more distinctly seen by grouping the curves into natural groups and taking the mean of their indications. Diagram No. 5 Plate I, gives the group of six curves from the surface to 30 fathoms, of four curves from 40 to 100 fathoms, both inclusive of 200, 300, and the single curve at 400.

Similar groups are shown on Diagram No. 6, Plate I, from Cape Henry, the cold wall, three maxima of temperature and three minima being very distinctly seen. The results of three different explorations of this section, by three different officers, in three different years, are shown upon the same diagram. The coincidence of result could hardly be better. The average of the whole of the observations is shown in No. 6 bis, Plate I.

The cold wall here gives a change of 22½° in 50 miles from the curves between 0 and 30 fathoms and 18° in 50 miles in the mean of 200, 300 and 400 fathoms.

The average of the three years comes out beautifully on Diagram No. 6 bis, Plate I. The Charleston curves are shown upon No. 7, Plate I. They are less regular than those just given, for reasons which will appear, when I come to speak of the second class of diagrams.

The conclusions deduced from the examination of all the sections between Cape Florida and Sandy Hook is, that the Gulf Stream is divided into alternate bands of hot, or warm and cool or cold water, the most distinct of which is that containing the axis of the Gulf Stream.

That between the stream and the coast there is a fall of temperature so sudden that it has been aptly called the cold wall, less distinct at the surface and where the overflow from the Gulf Stream passes furthest toward the shore, but still distinctly marked even at the surface.

Navigators have noticed these changes of temperature and have supposed themselves at each occurrence of warmer water to be in the hottest water of the stream and so have been greatly embarrassed and have deemed the phenomena and limits of the Gulf Stream to be very irregular.

The cold water between the Gulf Stream and the shore has also bands less regular than those beyond the axis of warmer and cooler water.

The intrusive cool water in the Gulf Stream on the Sandy Hook section was distinctly recognized in 1846 by Lieut. Geo. M. Bache, who from the facts observed, supposed it to represent a division of the warm water of the stream into two branches.

Passing through the Straits of Florida between the keys and reefs and the coast of Cuba we have after going beyond Cape Florida, a different type-curve. The cold wall is less distinctly marked and the rise of temperature is less marked. It rises however to an axis near the coast of Cuba. Throughout the length of the Strait there is but one maximum of temperature and the bands belonging to the Atlantic regimen do not occur in the straits. (See diagrams Nos. 3, 4, 5, 6, Plate II.) The cause of this change of regimen will be seen in presenting the other form of diagram.

(*b.*) *Curves of depths at the same temperature.*—I have selected curves from the southern portions of the work, partly because the bottom has been struck in the sections and the diagrams show its sections as well as those of the stream, and partly to show how fully the deductions in regard to the divisions of the stream, apply to these, as well as the more northern sections. The Charleston section of Lieut. Maffitt is given on diagram No. 9, Plate I. The surface curve, notwithstanding the disturbance by a storm, shows the cold wall, (see also No. 7) the axis and two other maxima, the corresponding minima, a maximum within the cold current which is not therefore, as has been supposed, cut off at Hatteras, the curve of 72° reaching to the coast and 77° nearly reaching it. The Cape Florida diagrams (Nos. 3 and 7, Plate II.) give two maxima with indications of a third and the corresponding minima. The cold wall cannot be recognized upon it, probably for the want of one or two, more positions.

The form of the bottom delineated on these two sections, namely the Charleston and Cape Florida sections, is remarkable and applies to the sections between them as far as explored. First is a gentle slope, then a sudden descent, a second steep pitch to a considerable depth, a range of hills, a valley and a second range.

The correspondence of these features with the bands of temperature is plainly marked. The cold water lies in the valleys and passing along the bottom rises upon the tops of the hills. The discovery of this range of hills was made at nearly the same time

by Lieut. Maffitt on the Charleston section and by Lieut. Craven on the St. Simon's section. Diagram No. 9, Plate I, shows this connection in a very striking manner as does also No.7, Plate II, and the figure of the bottom of the straits of Florida, shows why there are no bands formed prior to passing Cape Florida, in other words, why the regimen of the stream is different in the straits and in the Atlantic. In the straits we see (No. 9, Plate II) that after leaving the United States shore and the comparatively flat surface extending to the reefs, there is a rapid descent toward the Cuban side of the strait, the axis of the Gulf Stream being found in the deep hollow of that side of the strait.

These results, with a more elaborate discussion of them, were presented at the last meeting of the Association. It would seem from the configuration of the bottom, that the cold stream at the bottom of the straits of Florida divides, one portion passing to the north and west into the Gulf of Mexico and the other around the western end of the Island of Cuba. That the polar stream still occupies the bottom of the strait is shown by temperature of 35° Fahr. being reached at 600 fathoms from the surface off Havana.

Do these bands correspond throughout their length to the form of the bottom of the sea? This is not yet made out, many as have been the attempts to reach the considerable depths off the more northern sections. Three officers have attempted to sound out the Cape Cod section, but the cold wall is all that has been reached thus far. The range of hills nearest to the coast, has been traced from the coast of Georgia by Commander Sands to off Cape Lookout.

III. The Cold Wall.

The cold wall extends with varying dimensions and changes of its peculiar features, all along the coast where the stream has been examined. A diagram showing the features of the cold wall on the various Atlantic sections and those of the straits of Florida is given in No. 10, Plate I. Table No. 1 shows the distance of the cold wall from the coast and the dimensions of the Atlantic bands of the Gulf Stream.

The table shows that at Cape Florida and Cape Hatteras the cold wall is nearest to the coast. The distance of the axis of the stream from the coast will be found by adding half the numbers in the second column to those in the first column. It is obvious from these numbers, when taken in connection with the longitudes of the points where the sections originate, that the earth's motion is not the sole determining cause of the direction of the axis of the stream, a result which a more elaborate investigation of the movements from parallel to parallel confirms. In the portions of its course between Cape Florida and Mosquito inlet ($3\frac{1}{4}$° of latitude) the curve is actually slightly to the westward.

TABLE 1.—*Distance of the cold wall from the shore, and widths of the several bands of cold and warm water in the Gulf Stream, measured on the lines of the Sections.*

Names of Sections.	Distance of cold wall from shore in miles.	Width of 1st maximum or warm band.	Width of 2d minimum or cold band.	Width of 2d maximum.	Width of Gulf Stream, proper.	Width of 3d minimum or cold band.	Width of 3d maximum or warm band.	Width of 4th minimum or cold band.
Sandy Hook........	240	60	30	37	127	60	50	In 'ef.
Cape May..........	125	55	30	40	125	70	65	70
Cape Henry........	95	45	32	47	124	80	60	50
Cape Hatteras......	30	47	25	45	117	37	75	70
Cape Fear..........	60	30	20	37	87	30	60	25
Charleston..........	62	25	15	30	67	26	35	—
St. Simons..........	87	25	13	20	58	25	25	—
St. Augustine........	70	20	13	12	47	22	20	—
Cape Cañaveral.....	35	20	—	—	35	14	12	—
Cape Florida.......	10	25	—	—	25	5	—	—

Note.—The width of the bands beyond the 2d maximum, and north of Cape Hatteras are somewhat indefinite.

The table shows a width in the Gulf Stream proper along the Atlantic coast of from 25 miles off Cape Florida to 127 miles off Sandy Hook. The warm water at say fifteen fathoms, is from 30 to 150 miles in width. The stream widens each way from Cape Florida. These several divisions of the Atlantic stream lose a portion of their distinctness as we pass northward and eastward, the stream widening.

IV. Limit of Accuracy of the Determinations.

There are two modes by which the limits of accuracy of these results may be tested, by one of which their permanency is also tried. In this latter mode the sections are run over in different years, or in the same year by different officers, so as to connect the observations of one year with those of the next, or of one officer with that of another. Table No. 2 shows that the relative results are reproduced from year to year with less variability than those of the mean temperature of the section ; and hence the permanency of the bands and the possibility of observing them with the requisite precision must be admitted. On the Cape Henry section which was explored three times, the position of the cold wall and of the axis of the stream were reproduced within 5½ miles and those of the succeeding points of maximum and minimum temperatures within 7½ miles. As the positions at sea are liable to an uncertainty of some three to five miles it must be admitted that the permanency of the bands and the accuracy of the observations of them are fully proved.

The Cape Henry section was run over by Lieuts. G. M. Bache, S. P. Lee, and Richard Bache, the Hatteras section by Lieuts. Richard Bache and J. N. Maffit, and the Charleston section by Lieuts. J. N. Maffit and T. A. Craven.

Table 2.—*Table showing the probable uncertainty in the determination of maximum and minimum points, by running the same section over in different years by different observers.*

Cape Henry Section.

Dates and names of observers.	Mean distances from the shore in miles from the curves representing the groups.						
	Cold wall or 1st min.	Axis or 1st max.	Second min.	Second max	Third min.	Third max	Fourth min.
Lt. G. M. Bache, 1846	93	135	187	218	260	82	369
" S. P. Lee, 1847	91	146	185	215	2[illegible]1	83	338
" R. Bache, 1848	97	146	180	19	287	328	7
Means for three years.	84	142	184	210	279	3 8	370
Probable error for each year.	5·85	4·27	2·42	7·62	11·51	5·71	7·18

Cape Hatteras Section.

Lt. R. Bache, 1848	—	90	134	162	214	286	355
" J. M. Maffit, 1853	—	75	125	157	211	256	322
Means for two years	—	82	129	159	212	266	338
Probable error for each year	—	6·4	4·3	2·4	1·5	15	16
Means for both sections	5·85	5·3	3·4	5·0	6·4	1·04	1·16

Average uncertainty of maxima and minima, 6·9 miles.
" " cold wall and axis, 5·5 "
" " all the other points, 7·4 "

The other mode of testing the result is by the comparison of the remarkable points in the different sections, each one belonging to a different position and therefore being entirely independent of the other in its determination. It is established as a general law that this cold wall and axis of the hottest water change their position from the surface to the depth of six hundred fathoms slowly and by an ascertained progression, and that the succeeding maximum and minimum points are at the same distance from the shore, nearly, at different depths, or in a vertical line at all the different depths. The positions of these points as shown by the observations at different depths become thus the test of the permanency of their positions and of the accuracy with which they have been ascertained. Table 3 gives the probable error of the mean of the determinations of each point including the cold wall minimum, the axis maximum, and the successive minima and maxima to the fourth minimum inclusive. These results show that the cold wall minimum is ascertained, on the average, within 0 83 mile, the axis maximum within two miles and a half, the second minimum within two miles and a half, the second maximum and third minimum and third maximum, within four miles, and the fourth maximum within eight and a half miles, all being satisfactory except the last, which of course is in reality loosely defined. The Hatteras result for the axis of the stream, makes the probable error considerably larger than it would otherwise be, probably from the

fact that the proximity of the bottom of the sea, makes the result less permanent than in the other cases. Without this result the mean probable error would be 1·1 mile.

TABLE 3.—*Recapitulation, showing the value of the probable error of determination of the bands for each section and the average of the whole.*

Sections.	1st min.	1st max	2d min.	2d max.	3d min	3d max	4th min.
	Probable errors.						
Sandy Hook		·75		3·94	7·99		
Cape May	·82	1·25	2·54	1·57		4·03	4·87
Cape Henry, 3 years	·84	·61	·55	1·70	1·06	·94	3·42
Cape Hatteras, 2 years		6·77	6·36	9·31	5·69	6·23	
Cape Fear		1·25			2·98	3·49	13·37
Charleston	1·25	1·57	·72	2·09	2·40	·82	
St. Simons		·74	1·27	·41			
St. Augustine	·52	·51	·44	·44	·55		
Cape Cañaveral	·95	1·69	·39				
Mean probable error	·83	2·49	2·49	4·00	4·01	3·71	8·45

While these results are so permanent, the mean temperatures of the sections change considerably from year to year. The average temperature between the surface and 400 fathoms beyond, or outside of the cold wall on the Sandy Hook section in 1846, was as high as that on the Cape Henry section in 1848, and that on the Cape Fear section in 1853, within a degree of that of the St. Augustine section in 1853, while the Cape Hatteras section in 1848 and in 1853, differed two degrees in mean temperatures. Again the temperatures from the surface to 30 fathoms just below the axis of the stream in the Sandy Hook section in August 1846 was either as high or higher than those on the Cañaveral section in June 1853. In general the Cape May section in 1846 and the mean of the Cape Henry section of 1846, 1847 and 1848 are warmer at the same depths than the sections south of it were in 1848 and 1853.

These results show that there are great changes in temperature from year to year, and probably from season to season. Some progress has been made in connecting these results in a general way with the changes of weather in the Gulf of Mexico.

The depths at which the results are easily determined and where they are characteristic and as permanent as the phenomena permit is thirty fathoms.

V. FIGURE OF THE BOTTOM OF THE SEA, BELOW THE GULF STREAM.

We have seen that in cross sections there is a great resemblance in the bottom of the sea off our coast to the region of land more removed from the coast-line in the interior. The top of the first range of hills, (see Diagram No. 9, Plate I,) is 1500 feet above the valley to the eastward of it, distant 12 miles; and the top of the second range 600 feet above the same valley, distant 15 miles. The first slope is 125 feet, and the second is 40 feet,

to the mile. The bottom of the sea from the Tortugas section to that of Cape Florida, rises from 800 to 325 fathoms, and from the same point descends, in passing northward and eastward. The Cape Florida section showed that there then was present a ridge of comparatively cold water since the division into bands should apply along the stream as well as in the direction of its cross sections. The temperature of 40° is in fact reached on that section at 300 fathoms, and, as well as can be judged from the results in the separate sections there are divisions of this sort. The diagram No. 2, Plate II, shows where the curves of 50° and 45° are found upon the different sections and indicates a rise on the Charleston section and a sharp descent from Charleston to Cape Fear.

VII. General features of the Gulf Stream.

Upon the general diagram now presented to the members, (Plate III,) the general features of the Gulf Stream are represented from the Tortugas to the Cape Cod Section. Passing along the Cuban coast the temperature in June was found to be about 84° or 8° above the mean temperature of Key West, as given by the Surgeon General's report. The current here is feeble, but sufficient to cause it to be sought by sailing vessels making to windward and even by steamers. Issuing from the straits of Bemini, the stream is turned northward by the land which confines and directs its course. Its effective velocity is not derived from difference of temperature, as the observations abundantly show, the greatest relative differences being in fact crosswise of the stream. The direction is here a little west of north and the velocity is from 3 to 5 miles per hour. The temperature bands now begin. The bottom of the sea which was one slope and counter slope, across the Florida Straits, is here corrugated; the depth instead of being unfathomable, as has heretofore been supposed, is but 325 fathoms, in which depth the two currents, from the poles near the bottom and from the Gulf at the top, must pass each other. While the surface water is above 80° that near the bottom is as low as 40°.

The stream just north of Mosquito inlet begins to bend to the eastward of north, and off St. Augustine has a decided set to the eastward. While flowing thus onward the warm water seeks the sides of the channel overflowing towards the coast of Florida, and towards the Bahamas, but not as rapidly as it moves on north. Between St. Augustine and Cape Hatteras the set of the stream and the trend of the coast differ but little, making 5 degrees of easting in 5 degrees of northing. At Hatteras it curves to the northward and then runs easterly, making about 3 degrees of northing in 3 degrees of easting. In the latitude of Cape Charles it turns quite to the eastward having a velocity of between one and one mile and a half the hour.

That this curve follows the general sweep of the coast under water, appears most probable, the coast line, the curve of 100 fathoms and the ranges of hills discovered by Lieuts. Maffitt and Craven all seem to indicate it. That the direction of the stream is given in a general way by the configuration of the bottom of the sea, it is hardly possible to doubt, while admitting that it receives modification from other, and perhaps more general, causes. The after progress of this mighty stream, and of its branches if it does divide, remains yet to be traced and and so also its heading in the Gulf of Mexico.

I forbear to mingle doubtful speculation upon causes, with the inductions in regard to temperatures, which it has been the object of these observations to supply and of this lecture to bring to your notice.

ART. XXIX.—*On Fermented and Aërated Bread, and their Comparative Dietetic Value;* by J. DAUGLISH, M. D.*

[Extracted from the London Medical Times and Gazette, vol. i, p. 468, 1860.]

SINCE the new process of preparing bread has been introduced, —a process which effects the raising of bread wholly by mechanical means, imparting to it the most perfect vesicular structure, while it leaves the constituents of the flour wholly unchan-

* As most of our readers are doubtless aware, Dr. Dauglish is the author of a new system of bread making that has excited considerable interest among chemists during the last twelvemonth.

An extended description of this method was read at the Aberdeen meeting of the British Association in Sept., 1859, by Dr. Odling, from whose paper we take the following extracts.

"It is well known that the vesicular character of ordinary bread results from the development of carbonic acid gas uniformly throughout a mass of fermenting dough, whereby a loose spongy texture is imparted to what would otherwise be a dense sodden lump of baked flour and water. In fermented bread the carbonic gas thus generated within the substance of the dough is a product of the transformation or degradation of one of the constituents of the flour, viz., of the starch or sugar.

"In the plan of Dr. Dauglish the carbonic acid is produced independently and superadded to the flour which consequently need not undergo any degradation whatever. Water charged with carbonic acid (common "soda water") is mixed under pressure with the flour and the resulting dough, which becomes vesicular when the pressure is removed, is divided into loaves and baked in the ordinary way.

"The advantages claimed for the new process, are, 1st. Its cleanliness. Instead of the dough being mixed with naked arms or feet, the bread, from the wetting of the flour to the completion of the baking is not, and scarcely can be touched by any one. 2d. Its rapidity. An hour and a half serves for the entire conversion of a sack of flour into baked loaves, whereas in the ordinary process, four or five hours are occupied in the formation of the sponge, and a further time for the kneading, raising, and baking of the dough. 3d. Its preventing deterioration of the flour. In making fermented bread from certain varieties of flour, not in themselves unwholesome, the prolonged action of warmth and moisture induces a change of the starchy matter of the flour into dextrine, whereby the bread becomes sodden and dark colored. This change is usually prevented by the addition of alum, which is indeed

ged and uncontaminated,—there has not been wanting those who doubt whether the process of fermentation, by which bread has been hitherto prepared, is not really beneficial in other respects than that of imparting the vesicular structure to it; whether, in fact, the changes which the constituents of the flour —especially the starch—undergo, are not essential to healthy digestion in the stomach.

Although I believe there are few members of the Medical profession who will be prepared to maintain that fermentation is beneficial, still, as some do hold such an opinion, and have asserted likewise that starch which has not undergone the fermentive process is wholly unfit for human food, I am desirous of stating what I believe are good reasons for rejecting the process of fermentation for the new one which I have introduced.

In order to dispose of the assertion that starch requires to be prepared by the fermentive changes to render it fit for human food, it is but necessary to remark, that the proportion which the inhabitants of the earth, who thus prepare their starchy food, bear to those who do not, is quite insignificant. Indeed, it would appear that the practice of fermenting the flour or meal of the cereal grains is followed chiefly by those nations who use a mixed animal and vegetable diet, while those who are fed wholly on the products of the vegetable kingdom reject the process of fermentation entirely. Thus, the millions of India and China, who feed chiefly on rice, take it for the most part simply boiled; and that large portion of the human race who feed on maize, prepare

an almost necessary ingredient in the manufacture of bread from glucogenic flour. But in operating by the new process, there is no time for glucogenic change to take place, and consequently no advantage in the use of alum, with any description of flour. 4th. Its certainty and uniformity. Owing to differences in the character and rapidity of the fermentation, dependent on variations of temperature, quality of the yeast, &c., the manufacture of fermented bread frequently presents certain vagaries and irregularities from which the new process is entirely free. 5th. The character of the bread. Chemical analysis shows that the flour has undergone less deterioration in bread made by the new, than in that made by the fermented process. In other words, the percentage of extractive matters is smaller. The new bread has been tried dietetically at Guy's Hospital and by many London physicians and has been highly approved of. It is well known that for some years past, the use of fermented bread in dyspeptic cases, has been objected to by members of the medical profession, the debris of the yeast being considered unwholesome and liable to induce acidity. 6th. Its economy. The cost of carbonic acid is alleged to be less than the cost of yeast. Moreover, in making fermented bread there is a small but necessary waste of the saccharine constituents, which is avoided by the new process. 7th. The saving of labor and health. It substitutes machine labor for manual labor of a very exhausting kind. The sanitary condition of journeymen bakers was investigated some time ago by Dr. Guy, and found to be most lamentable, from their constant night work and from the fatiguing and unwholesome character of their labor, particularly the kneading In a politico-economical point of view, the process is important as removing bread-making from a domestic manual work, to a manufacturing, machine work."

From the character of the apparatus, the process can only be used profitably on a large scale, and not in small bakeries. F. H. S.

it in many ways, but they never ferment it. The same is true with the potato-eater of Ireland, and the oatmeal-eater of Scotland. Nor do we find that even wheat is always subjected to fermentation; but the peculiar physical properties of this grain appear to have tasked man's ingenuity more than any other, to devise methods of preparing from it food which shall be both palatable and digestible. In the less civilized states, a favorite mode of dressing wheat grain has been, by first roasting and then grinding it. On the borders of the Mediterranean it is prepared in the form of maccaroni and vermicelli, while in the East it is made into hard thin cakes for the more delicate, and for the hard working and robust into thicker and more dense masses of baked flour and water. Even in our own nurseries wheaten flour is baked before it is prepared with milk for infants' food. The necessity of subjecting wheaten grain to these manipulations arises from its richness in gluten, and from the peculiar properties of that gluten. If a few wheaten grains are taken whole and thoroughly masticated, the starchy portion will be easily separated, mixed with the saliva and swallowed, whilst nearly the whole of the gluten will remain in the mouth in the form of a tough tenacious pellet, on which scarcely any impression can be made. A similar state of things will follow the mastication of flour. In this condition, the gluten is extremely indigestible, since it cannot be penetrated by the digestive solvents, and they can only act upon its small external surface; hence the necessity to prepare food from wheat in such a manner as shall counteract this tendency to cohere and form tenacious masses. This is the object of baking the grain and flour as before mentioned, of making it into maccaroni, and of raising it into soft spongy bread; by which latter means the gluten assumes a form somewhat analogous to the texture of the lungs, so that an enormous surface is secured for the action of the digestive juices; and this I believe is the sole object to be sought in the preparation of bread from wheaten flour.

Wheat is said to be the type of adult human food. It supplies, in just proportions, every element essential to the perfect nutrition of the human organism. And yet in practice we find that the food which we prepare from it, and furnish to the inhabitants of our large towns and cities, is quite incapable alone of sustaining the health and strength of any individual. This is the more remarkable, since in Scotland we find that the food prepared from the oat, a grain possessing the same elements of nutrition as wheat, though in a coarser form,—furnishes almost the exclusive diet of a very large number of the hardiest and finest portion of the population.

In the large towns of France wheaten bread certainly forms a very large proportion of the diet of the laboring classes, but not

so large as oatmeal does in Scotland. And yet it has been remarked by contractors for public works on the Continent, that the chief reason why the Englishman is capable of accomplishing double the work of a Frenchman is, that the one consumes a very large proportion of meat, while the diet of the other is chiefly bread. In Scotland, however, the laboring man is capable of sustaining immense fatigue upon the nourishment afforded by oatmeal porridge.

The deficiency in wheaten bread in affording the nourishment due to the constituents of the grain, is to be attributed solely to the mode of preparing the flour, and the process followed for making that flour into porous bread.

The great object sought after both by the miller and the baker, is the production of a white and light loaf. Experience has taught the miller that the flour which makes the whitest loaf is obtained from the centre of the grain; but that the flour which is the most economical, and contains the largest portion of sound gluten, is that which is obtained from the external portion of the grain. But while he endeavors to secure both these portions for his flour, he takes the greatest care to avoid as much as possible, by fine dressing, etc., the mixture with them of any part of the true external coat which forms the bran, knowing that it will cause a most serious deficiency in the color of the bread after fermentation.

It is generally supposed that the dark color of brown bread —that is of bread made from the whole wheaten meal—is attributable to the colored particles of the husk or outer covering of the grain. But such is not really the case. The colored particles of the bran are of themselves only capable of imparting a somewhat orange color to bread, which is shown to be the fact when whole wheaten meal is made into bread by a process where no fermentation or any chemical changes whatever are allowed to take place. Some few years since, a process was invented in America for removing the outer seed coat of the wheat grain without injuring the grain itself, by which it was proposed to save that highly nutritious portion which is torn away, adhering to the bran in the ordinary process of grinding, and lost to human consumption. The invention was brought under the notice of the French Emperor, who caused some experiments to be made in one of the government bakeries to test its value. The experiments were perfectly satisfactory so far as the making of an extra quantity of white flour was concerned, but when this flour was subjected to the ordinary process of fermentation and made into bread, much to the astonishment of the parties conducting the experiments, and of the inventor himself, the bread was brown instead of white. The consequence, of course, has been that the invention has never been brought into practical operation.

It has been estimated that as much as ten or twelve per cent of nutritious matter is separated adhering to the bran, which is torn away in the process of grinding, and until very lately this matter has been considered by chemists to be gluten. It has, however, been shown by M. Mège Mouriès to be chiefly a vegetable ferment, or metamorphic nitrogenous body, which he has named Cerealin, and another body, vegetable Caseine.

Cerealin is soluble in water, and insoluble in alcohol. It may be obtained by washing bran, as procured from the miller, with cold water, in which it dissolves, and it may be precipitated from the aqueous solution by means of alcohol; but, like pepsin, when thus precipitated it loses its activity as a solvent or ferment.

In its native state or in aqueous solution, it acts as the most energetic ferment on starch, dextrine, and glucose, producing the lactic and even the butyric changes, but not the alcoholic.

It acts remarkably on gluten, especially when in presence of starch, dextrine or glucose. The gluten is slightly decomposed at first, giving ammonia, a brown matter, and another production which causes the lactic acid change to take place in the starch and glucose. The lactic acid thus produced immediately combines its activity with that of the cerealin and the gluten is rapidly reduced to solution.

The activity of the cerealin is destroyed at a temperature of 140° Fah., according to M. Mouriès, but my own experiments show that it is simply suspended even by the heat required to cook bread thoroughly; thus bread made without fermentation, of whole wheaten meal, or of flour in which there is a large proportion of cerealin, will, if kept at a temperature of about 75° to 85° Fah., pass rapidly into a state of solution, if the smallest exciting cause be present, such as ptyaline or pepsin, or even that small amount of organic matter which is found in impure water—while the same material, when it has been subjected to the alcoholic fermentation, will not be affected in a like manner.

The activity of cerealin is very easily destroyed by most acids, also by the presence of alum; and while it is the most active agent known in producing the earlier changes in the constituents of the flour, it cannot produce the alcoholic, but as soon as the alcoholic is superinduced the cerealin becomes neutralized and ceases to act any longer as a solvent. M. Mouriès, taking advantage of this effect of alcoholic fermentation, has adopted a process by which he is enabled to separate from the bran all the cerealin and caseine which are attached to it. He subjects the bran to active alcoholic fermentation, which neutralizes the activity of the cerealin, and at the same time separates the nutritious matter; and then having strained this through a fine seive, he adds it to the white flour in the preparation of white bread, by which an economy of ten per cent is effected, and the color of the bread is not injured.

The peculiar action of cerealin as a special digestive solvent of the constituents of the flour—gluten and starch—has been practically tested by Mr. Stephen Darby, of Leadenhall-street, in a series of careful experiments. He found that when two grains of dry cerealin were added to 500 grains of white flour, and the whole digested in half-an-ounce of water at a temperature of 90° for several hours, ten per cent more of the gluten, and about five per cent more of the starch, were dissolved than when the same quantity of flour was subjected to digestion without the addition of cerealin, but in which of course there was a small amount of cerealin that is present in all flours. The action of cerealin upon the gluten of wheat is precisely similar to that of pepsin on the fibrine of meat. Pepsin, acting alone on fibrine dissolves it, but very slowly, but if lactic acid be added solution takes place very rapidly. In like manner the starch present with the gluten of wheat is acted upon by the cerealin, and produces the necessary lactic acid to assist in the solution of the gluten by cerealin.

With the knowledge thus obtained of the properties of this substance cerealin, it is not difficult to understand why the administration of bran-tea with the food of badly-nourished children, produces the remarkable results attributed to it by men both experienced and eminent in the Medical profession; and why, also, bread made from whole wheaten meal, which contains all the cerealin of the grain, should prove so beneficial in some forms of mal-assimilation, notwithstanding the presence of the peculiarly indigestible and irritating substance forming the outer covering of the grain.

It will be seen that in all the methods of bread-making hitherto adopted, the peculiar solvent properties of this body, cerealin, have been sought to be neutralized simply because it destroys the white color of the bread during the early stages of panary fermentation. It is by thus destroying the activity of the special digestive ferment which Nature has supplied for the due assimilation by the economy of the constituents of the wheaten grain, that wheaten bread is rendered incapable of affording that sustenance to the laboring man which the Scotchman obtains from his oatmeal porridge. Although the new bread has been as yet but little more than experimentally introduced to public consumption, I have already received from members of my own profession, who have recommended it in their practice, as well as from non-professional persons, accounts of the really astonishing results that have followed its use in cases of deranged digestion and assimilation. Private gentleman have sought interviews with me to record the history of their recovery to health, after years of suffering and misery, by the simple use of the bread as a diet. Children that have been liable to convulsive attacks from

an irritable condition of the alimentary canal and nervous system, have been perfectly free from them immediately the new bread was substituted for fermented bread. And cases are now numerous that have been communicated to me by medical men of position, in which certain distressing forms of dyspepsia, which had remained intractable under every kind of treatment, have yielded as if by magic almost immediately after adopting the use of the aërated bread.

The delicate flavor of the new bread renders it peculiarly grateful to the stomachs of invalids and children, as well as of those whose tastes have not become vitiated by the habitual use of baker's bread, which is slightly sour, and tastes of yeast. The new bread was supplied to two wards in Guy's Hospital in place of the ordinary bread (which is of a very fine quality, made on the premises,) for two months, and in no case were there any pieces left in the wards unconsumed, while of the fermented bread large quantities of scraps were collected daily, for the consumption of which the appetites of the patients have been deficient.

That persons who have been long used to the strong yeasty-flavored bakers' bread should consider the new bread tasteless at first is not to be wondered at, since the delicate sense of taste is of all other senses the most easily lost by rough usage. Hence the argument put forth in defence of adulteration by some London tradesmen, especially the beer sellers, that the public will not buy the pure article, as it is wanting in the flavor to which they have been accustomed; and hence, also, the dislike of the Viennese of the fresh oysters supplied to them when the railway was completed, as they deemed them insipid, after the habitual use of oysters slightly decomposed, with which they had been supplied when it required a lengthened period to transport them from the sea.

I am disposed to attribute the beneficial effects of the new bread to two causes. The one to the *absence* of the prejudicial matters imparted to ordinary bread by the process of fermentation, and the other to the *presence* in the bread, unchanged, of that most essential agent of digestion and assimilation, cerealin.

I believe the prejudicial matters imparted to bread by fermentation to be chiefly two—acetic acid and the yeast-plant. The first is produced in large quantities, especially in hot weather, by the oxydation, by atmospheric contact, of the alcohol produced. The second is added when the baker forms his sponge, and is also rapidly propagated during the alcoholic fermentation, and cannot of course be afterwards separated from the other materials in the manner that the yeast and the other *débris* of fermentation separate themselves from wine and beer by precipitation in the process of fining. Nor is the life of the yeast-plant gene-

rally destroyed in baking, because it requires to be retained at the boiling point for some time before it is thoroughly destroyed; and bread is generally withdrawn from the oven, for economical reasons, even before the centre of the loaf has reached the temperature of 212°. It is not difficult to understand how the most painful and distressing symptoms and derangements may follow the use of bread in which the yeast-plant is not thoroughly destroyed previous to ingestion, and in those cases of impaired function in which the peculiar antiseptic influence of the stomachal secretions is deficient, and is incapable of preventing the development of the yeast-plant in the stomach, and the setting up of the alcoholic fermentation to derange the whole process of digestion and assimilation.

The presence of cerealin in bread is as beneficial as that of acetic acid and the yeast-plant is prejudicial. Digestion, or the reduction of food is evidently essentially dependent on the action of a class of substances which chemists, for want of a better term, have called ferments—to these substances belong pepsin, ptyaline, emulsin, diastase and cerealin; these are evidently types of a very numerous class, which act by producing those molecular changes in organic substances in which digestion consists; and since the purpose of digestion or solution is to prepare from heterogeneous substances taken as food a chyle, which shall not only when absorbed present all the elements of healthy blood, but shall previous to absorption, possess the properties which will constitute it the proper stimulus to the functional activity of the lactaels, it would appear to be necessary that each distinct substance taken as food should be furnished, not with its simple chemical solvent, but with that peculiar form of solvent or ferment which alone can carry it through those molecular changes which shall terminate in the production of healthy chyle. Hence we should infer that a substance was digestible or indigestible just in proportion to the provision that is made for its reduction to the standard of healthy chyle, and that substances which have hitherto been incapable of affording any nutrition whatever, may at some future day be rendered highly nutritious, simply by adding to them suitable ferments, artificially obtained or otherwise, that shall secure their passage through the proper molecular changes. Indeed, I think this subject opens up to us that very wide field of inquiry, as to whether the cause and prevention of disease, and the beneficial administration of remedies may not, for the most part, if not entirely, be dependent on the action of substances analogous to such bodies as ptyaline, pepsin, cerealin, etc., acting in concord with, or retarding and opposing the vital functions of tissues; and that by more profound inquiry in this field of research, the physiologist and the pathologist may not at a future day lay the foundation of true scientific Medicine.

Tunbridge Wells.

ART. XXX.—*Additional Note on the Potsdam Fossils;* by E. BILLINGS.

SINCE my note to Mr. Bradley's paper was written, he has collected quite a number of new specimens of *C. minutus* at the same locality. At his request I have examined them and find that they exhibit several of the parts not preserved in those upon which the original description was founded.

Fig. 4, (nat. size).

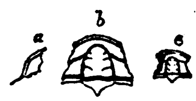


Fig. 4.—*a*, A detached cheek showing the small spine of the posterior angle. *b c*, Two specimens of the glabella, showing the spine on the neck segment.

1. The posterior angles of the head are produced into short spines, as we supposed, but these spines, instead of being elongate-triangular are sub-cylindrical or needle-shaped and projected outwards at an angle of 45° or thereabouts, to the longitudinal axis of the body. The cheek does not appear to be striated but rather smooth. These two characters furnish additional grounds for separating the species from *C. antiquatus* (Salter), which has the cheeks striated and the posterior angles of the head only slightly produced into short broadly triangular terminations.

2. The neck segment bears a short broad-based spine. The first specimens collected do not exhibit this, but on reëxamining them I think I can see traces of it. Some of the specimens of *C. coronatus* (Barrande) lately collected in the Primordial Zone of Spain have a spine on the neck segment of the same form as that of *C. minutus*, while others (according to the figures) have not; and it may be that individuals of our species will yet be discovered in which the absence of the spine can be clearly established. This remark is made here because on comparing the figures of the Bohemian and Spanish specimens of *C. coronatus* it would appear that the presence or absence of a spine on the neck segment is not always of specific importance and should some of those from Keeseville turn out to have only a plain neck segment we would not perhaps on that ground alone be authorized to constitute two species.*

* Compare the article, *Sur l'existence de la faune primordiale dans la chaine cantabrique*, par M. Casiano de Prado; suivie de la *Description des fossiles*, par MM. de Verneuil et Barrande. Bulletin Geol. Soc. France, 2e Series, vol. xvii, p. 516, (1860). And also Barrande's *Système Silurien*, plate 13.

3. Mr. Bradley's new specimens also show that there are three pairs of glabellar furrows, the anterior being represented by two small indentations just in advance of the points where the ocular ridges reach the glabella; and further that the course of the facial suture is the same as it is in *C. striatus*, (Emerich). The pygidium is more obtusely rounded than is represented in our Figure 2.

As to the correctness of the generic reference of this species it may be remarked that Barrande is of opinion that no less than eleven of those which Angelin has figured under the genera *Solenopleura*, *Eryx*, *Conocoryphe* and *Harpides* should be placed in *Conocephalites*. In this view of Barrande's, Angelin has concurred.* The genus has thus been greatly extended and judging from the form of the head (and more particularly of the glabella) of Angelin's species *C. brachymetopus*, *C. homelotopus* and *C. canaliculatus* I think we are perfectly justified in referring this species to *Conocephalites*. The genus is most closely allied to *Calymene*, having the same number of segments in the thorax —the same number and arrangement of pieces in the head and the same general form and lobation of the glabella, the differences between the genera consisting principally in certain characters of the pleuræ and hypostoma† to which may be added the ocular ridge which although not a constant character in *Conocephalites* may be regarded as of some generic value as it does not occur at all in *Calymene*. I would also state that since examining Mr. Bradley's recent collection, I have been strongly impressed by the resemblance between the form of the cheek and small needle-shaped posterior spines of *C. minutus* and the same parts of the head of the Quebec species which I have called *Menocephalus globosus*, and it appears to me that *Menocephalus* must be regarded as another closely allied genus. If we except those two genera, *Calymene* and *Menocephalus*, there is no other but *Conocephalites* to which our new trilobite bears any near affinity.

* See Barrande's "*Parallèle entre les dépôts Siluriens de Bohême et de Scandinavie*, p. 19; and compare the tables on p. 17 and 35 of the same work. See also Angelin's *Palæontologia Scandinavica*.

† See Barrande, "*Système Silurien du centre de la Bohême*," p. 417–419.

Montreal, 20th Sept., 1860.

ART. XXXI.—*The Great Auroral Exhibition of Aug. 28th to Sept. 4th,* 1859.—6TH ARTICLE; by Prof. ELIAS LOOMIS.

SINCE our last Auroral article was prepared for the press, the following letter has been received from the Secretary of the Smithsonian Institution.

Washington, June 6th, 1860.

Dear Sir:—Some time since, you wrote us in regard to the aurora of September, 1859, and I now write to inform you that we have a very large collection of materials in regard to this interesting meteor which in justice to the writers ought to be published. It is, however, a pity that the data for scientific deductions in regard to this interesting phenomenon should be scattered, and we will therefore present the whole to Silliman's Journal, provided the Editors will publish it. Will you take charge of it, and prepare it for the press?

JOSEPH HENRY, *Secretary S. I.*

The editors of this Journal have accepted the liberal proposition of Prof. Henry, so far as to publish whatever might be supposed to be of importance in an investigation of the theory of the Aurora. A considerable portion of the materials collected by the Smithsonian Institution have already appeared in former numbers of this Journal, and many of the reports are from neighboring stations where the Auroral appearances are almost identically the same. From the entire mass of materials we have aimed to select those Reports only which were the most complete and elaborate, and which were so distributed in geographical position as to afford a correct idea of the appearances throughout the entire area of the United States.

OBSERVATIONS OF THE AURORA OF AUGUST 28TH, 1859.

Selected from the Smithsonian Papers.

1. *Observations at Burlington, Minnesota,* (lat. 47° 1′, long. 92° 30′), *by* A. A. HIBBARD.

The aurora of Aug. 28th, commenced at 8 P. M., and increased very rapidly until 8½ P. M., when it came to a centre directly over our heads. It went about three-fourths the circumference of the horizon at the base, and completely round at the top, and down about one-third on the south side. Stars very bright and light enough to read very fine print. At 9 P. M. it had nearly disappeared over head; and but few beams to be seen in any part of the horizon, although new beams were forming in the north. At 9½ P. M. the beams were very light, and somewhat scattered. At 10 P. M. it had entirely disappeared.

2. *Observations at Marquette, Michigan*, (lat. 46° 32′, long. 87° 41′), *by* Dr. G. H. Blaker.

Aug. 28th, at 8 P. M., a bright crown overhead, with beams or streamers extending to every point of the compass, but soon lost in a white haze. In the course of an hour these streamers extended to the horizon in every direction, with bright streams of white light shooting up towards the crown, all of which became perfectly red, or of a bright crimson color over head. This continued to increase in brightness until after midnight, with floods of white light at the horizon, all passing into crimson fleecy vapor in the zenith.

3. *Observations at Winona, Minnesota*, (lat. 44° 3′, long. 91° 36′), *by* T. F. Thickstun.

Aug. 28th, an auroral bank and a few pencilled streamers had formed at 9½ P. M. At midnight the streamers and corona filled the whole heavens except the N. E. portion. During the whole night the light was equal to that of a half moon.

4. *Observations at Green Bay, Wisconsin*, (lat. 44° 30′, long. 87° 56′), *by* D. Underwood.

Aug. 28th, about 7½ P. M., the aurora was visible in the northern part of the heavens, but did not attract particular notice until about 9 P. M. Soon after eight the sky began to redden, and became nearly of a blood-red color. Soon the streaks were observed shooting upward from all points of the horizon, and concentrating in a large luminous mass in mid-heavens. The greatest intensity of color was at the zenith. Rays were constantly shooting up from all points of the horizon and the colors constantly changing. The rays emitted an intense red light for about half an hour, when they began slowly to fade away in the north and south, but in the east and west they continued to glow until 10 P. M., when they began to fade away. Flashes of white light appeared among them, commencing from the horizon and moving upwards, following each other in rapid succession like the waves of an immense sea of light. They grew brighter as the red color disappeared, and when this was wholly gone they also gradually faded away.

5. *Observations at Milwaukee, Wisconsin*, (lat. 43° 3′, long. 87° 57′), *by* Prof. E. P. Larkin.

Aug. 28th, at 8 P. M., an aurora commenced. About 8½ P. M. an arch formed from S.W. by the north to the S.E., with dark broken clouds below. The streamers now commenced, principally in the N.W. and N.E., and were surprisingly beautiful, of crimson, purple, peach and orange; the crimson predominating. At $8^h 45^m$ a perfect corona formed a few degrees south of the zenith.

At 9 P. M. the aurora began to fade, and at ten had nearly disappeared. At $10\frac{1}{2}$ P. M. the north gave indications of another aurora which occurred about 12 o'clock, nearly equal in splendor to the first, and still another occurred about 3 A. M. There were also auroras late in the night of the 30th and also of the 31st.

6. *Observations at Burlington, Wisconsin,* (lat. 42° 39′, long. 87° 44′), *by* D. MATHEWS.

Aug. 28th, at 8 P. M., the appearance was that of a large luminous ring surrounding the zenith; but this form was very transient, the light becoming concentrated in the west. Between 8 and 9 P. M. there were two arches formed in the north, the first almost 30° in altitude, and the second about 40°. From the outer edge of the larger arch darted a succession of streamers or rays of light. At $8^h\ 15^m$ a perfect flood of light came up in the east, not in streams, but like the dawn of day, just before sunrise. This appearance lasted about half an hour. At 9 P. M. streams of light radiated in every direction from a point about ten degrees south of the zenith, covering the whole heavens except a space in the south. One broad belt of red light extended from near the zenith to the horizon at a point a little north of west.

7. *Observations at Dubuque, Iowa,* (lat. 42° 30′, long. 90° 52′), *by* ASA HORR, M.D.

Aug. 28th, the aurora began with floating irregular masses of auroral clouds in the north, which soon spread over the sky, terminating in a broad zone of light spanning the heavens from E. to W. and reaching to 20° south of the zenith. At 8 P. M. many of the luminous clouds became distinctly crimson, with the deepest hue near the horizon. At $8^h\ 15^m$ a distinct arch formed in the north crowned with tall flickering white streamers. The crimson clouds now dissolved into paler streamers; at $8^h\ 35^m$ the streamers subsided; and at $8^h\ 40^m$ the red clouds near the horizon and the diffuse light in the north appeared again. From this time till 9 P. M. a crimson shade was spread over the whole of the broad belt, with varying hue and brightness. The arch in the north formed again with whitish streamers which remained moderately bright until 11 P. M.

8. *Observations at Waltham, Massachusetts,* (lat. 42° 24′, long. 71° 14′), *by* REV. THOMAS HILL.

Aug. 28th, at $7\frac{1}{2}$ P. M., there were visible some splendid masses of rose-colored light in the east and west near the horizon; that in the west being nearly obscured by twilight. At $7^h\ 45^m$ a well defined arch passed south of the zenith, and all the sky north of it was filled with light, radiating toward the pole of the

dipping needle. I watched the aurora from 9 to $10\frac{1}{2}$ P. M., and at 3 A. M. got up again to look. It was then very brilliant and rosy all along the southern horizon.

9. *Observations at Monroe, Michigan,* (lat. 41° 56′, long. 83° 22′), *by* Miss HELEN J. WHELPLEY.

Aug. 28th, about $8\frac{1}{2}$ P. M., there was a broad line of intensely yellow light extending from east to west. In a few moments the whole sky to within 20° of the south, seemed to be permeated with a clear whitish light; yellowish rays constantly shooting up from the east, north and west horizon to the zenith. At the same time appeared a bright rose colored mass in the east, which gradually enlarged until it covered nearly three-fourths of the heavens, and about 9 P. M. the rays met and formed a perfect corona a little south of the zenith.

10. *Observations at Willow Creek, Illinois,* (lat. 41° 45′, long. 88° 56′), *by* E. E. BACON.

Aug. 28th—aurora first seen at 8 P. M., corona and beams at $8^h 40^m$, arch $8^h 45^m$. Beams of red very brilliant in the east and west, and at the corona from $8^h 35^m$ to $8^h 55^m$. At 9^h the beams and corona had disappeared, and a broad red belt extended across the heavens, passing over to the south. Two distinct arches were formed in the north. At $9^h 15^m$ bright beams in the southeast, the red belt disappearing in the east. At $9^h 25^m$ redness nearly gone. At $9^h 28^m$ arches broken. At $9^h 35^m$ brilliant spot in N.E. At $9^h 40^m$ arch reformed, but not so brilliant. At $12^h 30^m$ beams with far greater grandeur than at $8^h 40^m$. Beams streamed from all round the horizon to the zenith. In the southern half of the sky, the beams flashed like the blaze of a great fire. At $12^h 45^m$ a bright belt from E. to W., in the south, with a dark belt something like a cloud under it. At $1^h 12^m$ bright belt in the south gone. Aurora lasted till daylight.

11. *Observations at Sandwich, Illinois,* (lat. 41° 31′, long. 88° 30′), *by* Dr. N. E. BALLOU.

Aug. 28th, at $7\frac{1}{2}$ P. M., there was a bright luminous band, a degree in width, spanning the heavens. Soon it became tortuous; and in the zenith it pointed southward half way to the horizon. At 8 P. M. red gleams shot up in the N.W. directly to the zenith. At $8\frac{1}{2}$ P. M. the same appearance sprung up in the N.E. At 9 P. M. the entire northern half of the sky was brilliantly red, with gleams which soon culminated in a corona. At $9\frac{1}{2}$ P. M. the whole was tinged with red, alternating with beams of light. At 10 P. M. the red tinge floated away to the south. At midnight it presented much the same appearance as before.

12. *Observations at Sag Harbor, New York*, (lat. 41° 0′, long. 72° 20′), *from the Sag Harbor Express.*

Aug. 28th—between 8 and 9 o'clock rays of clear, white light arose from every part of the horizon, and soon met in a common focus at a point about 15° S.E. of the zenith, amid the four stars forming the trapezium of the Dolphin. At 10 P. M. a more dense light appeared at two points on the horizon a little north of east and west. These were at first of a bright yellow light; but at 10½ P. M. that in the east assumed a dark crimson hue, and the light in the west was tinged with a paler red. At this time began the waves of undulating light, their motions being upward and across the radial columns in a direction from east to west.

At midnight the brightness of the columns increased, the undulations increased, especially in the south, and their motions became exceedingly rapid. The usual dark bank now appeared upon the horizon in the south, well defined, but occasionally broken by minor columns of light. The whole heavens continued thus glowing until the aurora was eclipsed by the light of day.

13. *Observations at Kanosha, Nebraska*, (lat. 40° 51′, long. 95° 44′), by BELA WHITE.

There was a very brilliant light of a pink color from about 1 A. M. Aug. 29th until daylight in the northern half of the hemisphere, shooting upwards to the zenith, and passing off to the south. It was so light as to enable a person to read coarse print.

14. *Observations at Great Salt Lake City, Utah*, (lat. 40° 45′, long. 111° 26′), *by* W. W. PHELPS.

Aug. 28th between 9 and 10 P. M. a palish light wavered up about 30° towards the zenith; thence it spread east and west with increasing grandeur, till about 11 P. M. when the perpendicular streams beautified a large portion of the northern hemisphere. The light about 20° above the horizon, decreased upward and downward, so that at nearly midway to the zenith, the light changed into a fine yellow green, which was joined by a rich livery of crimson, spanning the whole heavens from east to west, as a belt several degrees wide. This magnificent phenomenon continued in varying hues, until about 1 A. M.

15. *Observations at New York City*, (lat. 40° 43′, long. 74° 5′), *by* Prof. O. W. MORRIS.

Aug. 28, 8 P. M. there was a bright band above the horizon, which spread upwards with rays. At $9^h\ 10^m$ the light spread over the heavens, occupying the zenith and either side with white, and interspersed with pink colored portions. At $9^h\ 20^m$ it formed an arch in the south with a dark segment below, while the northern portion of the sky was perfectly clear. At $9^h\ 25^m$

the beams shot up from all sides to the zenith, forming a beautiful corona which lasted 5 or 8 minutes, sometimes putting on the appearance of conflicting waves. Nearly the whole sky was of a pink, and some portions of a dark red color. It faded gradually away, first disappearing in the south, and at 10 P. M. only a bright band across the northern portion without a dark segment. About 10^h 30^m occasional rays shot up to the zenith. At 11^h it was very light, so that objects could be seen at a distance. The aurora continued with varied colors and brightness until the dawn of the 29th.

16. *Observations at Pekin, Illinois*, (lat. 40° 36′, long. 89° 45′), *by* J. H. RIBLET.

Aug. 28, 8^h 20^m P. M. a white band running from N. W. to E. with two columns shooting up, one by the north star the other through Ursa Major. 8^h 30^m it is passing westward and a high column is passing about 5° east of the north star, and about 10° above it. At 8^h 40^m two columns passing nearly to zenith on the east side of north star. At 8^h 46^m the column by the north star increased in width, the top bent over forming a semicircle to west. Color white in north, changing to a rose at its upper edge, and a red in the east. At 8^h 55^m arches forming; the lowest about 10° above the horizon, and of a pale color. The second about 30° above the horizon, and of a pale orange color. Streamers running from the lower arch through it and about 30° above it. The color was most intense in the east. At 8^h 57^m both arches better defined. The streamers passing from the lower through the upper to a point about 10° west of the zenith. At 9 P. M. less brilliant and ceased observing.

17. *Observations at Urbana, Ohio*, (lat. 40° 6′, long. 83° 43′), *by* Prof. M. G. WILLIAMS.

Aug. 28th, at 9 P. M. columns of white and yellowish light shot from all points of the arch which extended from N. 80° E. to N. 75° W. Many of the corruscations passed beyond the zenith; in the east the light was pink and deep crimson forming a mass about 30° broad and 60° high reaching down to the horizon. The color was sometimes almost blood red. At 9^h 10^m a similar mass formed in the N.W. At 9^h 15^m a remarkably beautiful column shot up at N. 50° W. having a breadth of 10° and reaching to the zenith. The colors were white, yellowish, pink and crimson. About 9^h 20^m an arch was formed in the south, having an altitude of 40° at the centre. A few minutes later, the crimson light extended down from the zenith, quite to this arch, so that most of the sky was covered with colored light. At 9^h 50^m, a beam 2° broad, shot up from S. 80° W. passing 20° or 30° beyond the zenith. In a few minutes the beam seemed to be broken up into fragments of 5° or 6° in length, and presently

vanished. At 10 P. M. the brightness was much diminished; at 10^h 15^m revived; the northern arch very brilliant and slightly tinged with crimson. At 10^h 30^m the arch extended from E. to N. 85° W. At midnight the exhibition was still fine. From 2½ to 3 A. M coruscations of white light shot up all along the horizon from E. to S. 15° W. The aurora continued to decline, till the dawn of day.

18. *Observations at Henry Co., Indiana*, (lat. 40°, long. 85° 15′), *by* WILLIAM DAWSON.

Aug. 28th, about 9 P. M. a red cloud covered a large portion of the eastern sky with a similar one in the N.W. and several large luminous beams extended from the north point to the zenith. Soon the red disappeared, and the bright streaks grew much shorter, leaving a bright cloud brilliantly fringed with white, near the northern horizon. After some minutes, several small bodies, like long white clouds, started from about 25° S.E. of the zenith, and moved slowly towards the west, disappearing about the same distance nearly S.W. of the zenith. About midnight, a dark cloud decked with immense streamers of white, glaring light, rested on the northern horizon, when suddenly it burst forth into streaming corruscations of red, purple and white lights, shooting to a point 15° or 20° south of the zenith, where these flashing lights presented the appearance of a cloud, tinted with vermillion and purple. At 12½ A. M. fully two-thirds of the heavens were wrapt in flashing torrents of streamers directed towards this point. This tremulous illumination lasted about an hour, when it partly vanished; but soon the sky was again covered with darting streamers nearly as before, and the light seemed more vivid than at 12½ A. M. At 2 A. M. the light had in a great measure passed away.

19. *Observations at Gettysburg, Pennsylvania*, (lat. 39° 49′, long. 77° 15′), *by* REV. M. JACOBS.

Aug. 28th, the aurora was visible in the evening twilight especially to N. and N.E. At 9 P. M. the whole sky as far south as within 30° of the southern horizon was covered with alternate bands of luminous matter like cirrus cloud. At 9^h 25^m the luminous band within about 20° of the northern horizon began to shoot up streamers, and soon became a mass of streamers which filled the whole northern sky, darting up to a point about 2° east of Aquila, where they crossed, some radiating thence southward. The streams were visibly wafted round on the east and west, to the S.E., S.W. and even south. The streamers were mostly of orange, white below and crimson red above. At 10 P. M. the sky was mostly clear; the bank of vapor, dark below but luminous above, with a few streamers occupied the northern horizon, rising to the height of 20° or 25°. The light was equal to that of the moon at quadrature.

20. *Observations at College Hill, Ohio,* (lat. 39° 19′, long. 84° 26′), *by* Prof. J. H. Wilson.

Aug. 28th, the aurora commenced at 8 P.M. and continued with increased splendor till 1 A.M. The corona formed about 10 P.M. with arch and beams. At 1 A.M. the coruscations darted from the horizon upward to the zenith and about 20° beyond. The light was equal to the clearest full moon.

21. *Observations at Wyandott City, Kansas,* (lat. 39° 7′, long. 94° 44′), *by* John H. Millar.

Aug. 28th, at 11½ P.M. a diffused light was observed in the N.N.E. gradually increasing until at 1 A.M. the whole northern sky from N.W. by W. to S.E. by E. was covered with rays and streamers of moderate brightness, shooting up to within 30° of the zenith, and changing rapidly, from a uniform white to a tinge of purple. The aurora passed off about 1½ A.M.

22. *Observations at St. Louis, Missouri,* (lat. 38° 37′, long. 90° 15′), *by* A. Fendler.

Aug. 28th, at $8^h 30^m$ P.M. I observed in the northern part of the sky some large patches of a deep red color, and the horizon towards the north was filled with a white light. The white as well as the red light rose gradually up to 45°. At $8^h 35^m$ the upper part of the light to the right was of an unusually deep red, while that to the left was greenish white. The aurora increased in splendor till $8^h 50^m$, rising to the zenith, with red streamers running nearly N. and S. The aurora now declined in the north, but spread its red color to about 10° south of the zenith. By 9 P.M. the red color was gone, only a faint white light remaining near the northern horizon, which continued till $9^h 20^m$ when it became obscured by clouds.

Aug. 29th at 1 A.M. I awoke and found the aurora more brilliant than last evening. It reached 45° south of the zenith, and had a corona 15° or 20° south of the zenith, of a deep fiery red, sending rays in all directions from this point towards the horizon. Towards the north the light was white, and close to the northern horizon the sky was blue. By $1^h 20^m$ the aurora had retired to within 80° above the northern horizon.

23. *Observations at Fredericksburg, Virginia,* (lat. 38° 30′ long. 77° 30′), *by* Charles H. Robey.

Aug. 28th, a most brilliant light appeared 8 P.M. and disappeared about $4^h 15^m$ A.M. It was generally diffused over the face of the heavens with a brightness exceeding that of the full moon, the brightest part being N.E. and N.W. About N.E. it was of a red color.

24. *Observations at the eastern base of the Sierra Abajo, Utah,* (lat. 38°, long. 110°), *by* Dr. JOHN S. NEWBERRY.

Aug. 28th—being at an elevation of about 7000 feet above the sea, from 11 to 12 o'clock the aurora appeared remarkably brilliant, the entire northern heavens being covered with a diffused red flush, with flashes of deeper red and white light. The auroral flush was noticeable in the north before 10 o'clock, but was not conspicuous before 11.

25. *Observations at Santa Clara, California,* (lat. 37° 18′, long. 122° 0′), *by* OLIVER S. FRAMBES.

Aug. 28th, at 9 P. M., about 10° E. of north the sky seemed tinged with red light. In half an hour several columns of mellow light were formed which rose to a height of 40°, and the colors became very bright. The light gradually moved to the east, and after two hours or more gradually faded away.

26. *Observations at Paducah, Kentucky,* (lat. 37° 5′, long. 87° 21′), *by* A. MATTISON.

Aug. 28th, at midnight, the clouds cleared off and showed the most beautiful aurora I ever saw in this latitude. Sometimes it was red, and sometimes it sent up streamers overhead. Aug. 29th, 1 A. M., aurora very bright, but the sky became overcast. The light continued till near day break.

27. *Observations at Monterey, California,* (lat. 36° 36′, long. 122° 54′), *by* Dr. C. A. CANFIELD.

Aug. 28th—a very brilliant aurora from 9½ P. M. to 11 or 12 P. M.

28. *Observations at Raleigh, North Carolina,* (lat. 35° 40′, long. 78° 52′) *by* WILLIAM H. HAMILTON.

Aurora appeared at 9 P. M. and lasted till 11 P. M. As light as if the moon was shining.

29. *Observations at Dallas, Texas,* (lat. 32° 45′, long. 96° 46′), *by* JOHN M. CROCKETT.

Aug. 28th, at dark the northern sky had the appearance of bright twilight. It continued to brighten until it extended from N. to N. E., and assumed a red tinge, with columns perpendicular to the horizon, and extending more than half way up the heavens. About the time of the greatest illumination, a round body in the N. E., about 15° in diameter, became of a beautiful bright scarlet, which in 20 or 30 minutes moved slowly towards the north, displacing the columns as it went. The whole scene occupied about one and a half hours and was constantly changing. The aurora continued with diminished brightness till near daylight.

30. *Observations at Selma, Alabama*, (lat. 32° 25′, long. 86° 51′), *by* S. K. Jennings, M.D.

Aug. 28th, about 8 P. M., there was a very well defined arch of red fleecy looking clouds extending up about 20°, that was beautifully brilliant for half an hour. From 8½ till after 10 P. M. there were incessant flashes that might have been taken for diffuse lightning.

31. *Observations at Cahawba, Alabama*, (lat. 32° 19′, long. 87° 16′), *by* Matthew Troy, M.D.

Aug. 28th, from 8 to 9 P. M., a bright light was visible a little east of north. It was brightest near the horizon, and extended to a height of about 30°, gradually fading at its upper border.

32. *Observations at Jacksonville, Florida*, (lat. 30° 15′, long. 82° 0′), *by* Dr. A. J. Baldwin.

Aug. 28th, about 8 P. M. was seen a remarkable aurora which continued until 2 A. M. Aug. 29th. At times it was of a vivid red, with streamers radiating towards the zenith. The brightest were from N.W. to N.E. The color would almost fade out at times, and then lighten up the heavens again with a brilliancy which was majestic. About 9 o'clock there was a dark cloud in the north extending from N.W. to N.E.; and the auroral display was beautiful along the fringe of this cloud.

33. *Observations at Micanopy, Florida*, (lat. 29° 35′, long. 82° 18′), *by* James B. Bean.

Aug. 28, just after dark I noticed a luminous appearance in the north, which at times disappeared and then reappeared with increased brightness, till 9 o'clock, when it exhibited streamers shooting up toward the zenith, and sometimes a deep red glare of rosy light toward the N.E. At 10 P. M. the streamers were quite distinct, presenting beams of gray and purple light. It disappeared about 11 P. M. but reappeared with more beauty between 1½ and 2 A. M. The streamers were very distinct, exhibiting various colors, and shooting at times within 10° or 15° of the zenith. The luminous haze in the N.W. continued till it was obscured by day-light.

34. *Observations at Cedar Keys, Florida*, (lat. 29° 7′, long. 83° 2′), *by* Judge Aug. Steele.

Aug. 28th, occurred an aurora, brightening up the northern horizon with most beautiful coruscations. It extended from N. to N.E. and upward to about 30° above the horizon, and exhibited frequent streaks of reddish yellow, shooting upward in pointed forms of the most dazzling brightness. It ceased about 9½ P. M. but reappeared the next morning in still greater brilliancy and continued until overpowered by the light of day.

35. *Observations at Corpus Christi. Texas*, (lat. 27° 45′, long. 97° 30′), *by* A. M. Lea.

Aug. 28th, about 9 P. M. an aurora reddened the sky in the north, through about 90° of the horizon, and rising about 40° above it, with columns of light stretching from the horizon towards the zenith.

36. *Observations at Key West, Florida*, (lat. 24° 33′, long. 81° 48′), *by* William C. Dennis.

Aug. 28th the aurora was faintly visible soon after sun-down and did not increase materially in brightness till 8½ P. M. At 9 P. M. the color was of the most fiery red. The direction of the middle point of the aurora was N. 10° E. and both horizontally and vertically it subtended an angle of about 30°. There was a narrow line of immovable clouds along the northern horizon in the direction of the aurora. At 9 P. M. the aurora commenced fading rapidly, and had disappeared at 9½ P. M.

Observations on the Aurora of Sept. 1st and Sept. 2nd, 1859.

Selected from the Smithsonian Papers.

1. *Observations at St. Johns, Newfoundland*, (lat. 47° 35′, long. 52° 38′), *by* E. M. J. Delaney.

Sept. 1st, an Aurora of various colors appeared in the west, moved towards the zenith and disappeared.

Sept. 2d at 8 P. M. an Aurora of various colors appeared in the north, south and west, moved towards the zenith and disappeared.

2. *Observations at Burlington, Minnesota*, (lat. 47° 1′, long. 92° 30′), *by* A. H. Hibbard.

Sept. 2d, Aurora commenced at 8 P. M. in the north, and N.E.; increased very fast until 8½ P. M. when it formed a perfect arch frrom N.E. to S.W. and ran about one-third of the way down from the zenith on the south side. The east and west parts were very red; the east part flashy like lightning. At 9 P. M. nearly disappeared. At 9¼ P. M. commenced in the north with very bright and flashy light; then very bright streamers ran up instantly almost to the zenith. At 9½ P. M. nearly disappeared; only bright flashy spots E. and N.W.

Sept. 3d, Aurora commenced about 8½ P. M.; beams ran up very rapid but very dim; formed perfect at about 9½ P. M.

3. *Observations at Princeton, Minnesota*, (lat. 45° 50′, long. 93° 45′), *by* O. E. Garrison.

Sept. 2d, a bright aurora extending from the N.W. to the E. and culminating in the zenith. It was a bright display of streamers in bands varying from a bright white to a red flame

color; the portion on the eastern extremity was flashing rather than streaked; that on the west was of a reddish flame color. The bands were about two degrees wide reaching from the horizon to the zenith. The central portion of each band being brightest, diminishing to a slight light on the edges. The first appearance was at dark, and it was still bright at 10 P. M.

Sept. 3d, the Aurora was repeated, but much less brilliant and and only about 45° elevation with a dark arch beneath about 20° above the horizon. Disappeared before 9 P. M.

4. *Observations at Gardiner, Maine,* (lat. 44° 11′, long. 69° 46′), *by* R. H. GARDINER.

Sept. 1st, brilliant aurora over dark arch.

Sept. 2d, very remarkable aurora. Colored streamers with constant, and very brilliant flashes of light at north and east, and reaching south of zenith. At 9½ P. M. two very bright arches at the north, one about 12° the other about 25° above the horizon, the upper one being extremely bright; the sky between them and below the lowest, being of a dark purple.

Sept. 3d, very brilliant aurora with colored streamers in all parts of the sky.

5. *Observations at Ogdensburg, New York,* (lat. 44° 43′, long. 75′ 26°), *by* W. E. GUEST.

Sept. 2d, at 1 A. M. a splendid aurora. The light continued for nearly two hours.

Sept. 2d, 9½ P. M. A few faint streamers shot up in the east and at the same time there was a faint rose colored light in the west, when all at once there commenced on every side a display of waves of auroral light. It was an undulating motion commencing near the horizon, and waving up gradually toward the zenith. In ten or fifteen minutes it had reached the zenith, and a corona was formed, its rays of different lengths pointing downward. It disappeared almost as rapidly as it came on, and a faint light was spread on all sides. A few minutes before ten a large arch was formed, one extremity resting in the east, and the other in the N.W. Its base was dark, yet the stars were glittering through its whole length. The arch was somewhat irregular in form in the N.W. It rose gradually until a faint double arch was formed. The streamers were quite stationary, without any motion from right to left. In less than an hour it had lost its form, and the light was diffused throughout the glittering dome. There was some light continued through the night.

6. *Observations at Salem, Oregon,* (lat. 44° 58′, long. 123° 4′), *from the Statesman.*

Sept. 1st, about 8 P. M. a faint radiance was observed shooting up from the northern horizon, and gradually the whole heavens

from north to south were covered with a delicate rosy tint, bright and glowing in the zenith, and decreasing in brilliancy near the horizon. It was most brilliant about 11 P. M. at which time it yielded sufficient light to read common print quite easily. It continued to shine with gradually decreasing splendor, and finally yielded as morning approached.

7. *Observations ot Fort Umpqua, Oregon,* (lat. 43° 48′, long 124° 6′), *by* HENRY OATLEY, U. S. Army.

An aurora was observed on the night of Sept. 1st. The light was most intense about midnight, and was sufficient to enable one easily to read print.

8. *Observations at Rochester, New York,* (lat. 43° 8′, long. 77° 51′), *by* M. M. MATHEWS.

Sept. 2d, $1^h\ 45^m$ A. M. the southern sky was one entire sheet of red light, extending from near the zenith, quite down to the horizon, and reaching laterally from S.E. to N.W. At 2 A. M the redness gathered intensity, and divided off into two nearly equal portions, one occupying the S.E. and the other the N.W. section of the sky, and for half an hour assuming a deep cherry red hue, with an occasional streamer of white light ascending nearly or quite to the zenith.

At 3 A. M. the whole sky from N.W. around to the S.E. became one entire blaze of deep red, and began sending off from all portions of its lower margin, the most brilliant streamers of white light which waved and flickered in front of the dark red back ground. They converged to a point just south of the zenith. This corona was most distinct at $3^h\ 15^m$ A. M. when it presented the appearance of an immense fan resting on the horizon. At the north lay a heavy bank of cloud rising about 2° above the horizon, and during the most brilliant display at the south, the upper edge of these clouds was tinged with a most beautiful orange color. At $3^h\ 30^m$ A. M. the redness had become comparatively faint, and the streamers had entirely disappeared.

Sept. 3d, about 8 P. M. was another auroral display confined mostly to the northern sky, and consisting principally of white streamers that were constantly flickering and dancing. At 9 P. M. they reached a point south of the zenith, and were attended by flashes of extreme brightness. At 11 P. M. the light had become quite faint, and the the streamers had disappeared.

9. *Observations at Ostego, Michigan,* (lat. 42° 28′, long. 85° 42′), *by* MATTHEW COFFIN.

Sept. 2d, aurora brightest about 2 A. M. when there was a beautiful corona a little S.W. of zenith, and rapidly shooting rays from N.W. and N.E. meeting at that point.

10. *Observations at Riley, Illinois*, (lat. 42° 11′, long. 88° 33′), *by* E. BABCOCK.

Sept. 1st, 11 P. M. aurora displaying beautiful red and white streamers covering all the northern half of the heavens. At midnight the whole north was covered with beautiful streamers of varied colors. At 12^h 15^m A. M., streamers shot up from the north; the whole moved south and rested about 20° above the southern horizon, at which time a dark belt appeared under the white. Immediately streamers shot up from all around the horizon centering near the zenith. The deep red prevailed at S.E. and N.W. In less than two minutes, the whole became a broken mass, and the streamers disappeared. The dark belt still rested upon the southern horizon, and the light continued all over the heavens.

11. *Observations at Davenport, Iowa*, (lat. 41° 30′, long. 90° 38′), *by* H. J. FINLEY

Sept. 2d, 8 P. M. aurora in brilliant reddish parallel rays running east and west about 45° above the northern horizon. Corona pale and but few rays.

12. *Observations at Camp, No. 33, Nebraska*, (lat. 42°, long. 109° 50′) *by* Capt. J. H. SIMPSON.

Sept. 1st, at 11 P. M. about two thirds of the whole southern heavens appeared one sheet of beautiful roseate light. For a while, the light continued in a state of repose; the most concentrated portion forming a limiting belt on its northern side, and extending from a point on the horizon about 10° north of east, across the heavens to a point on the horizon about due west. From this belt, the light with its roseate hue was diffused southwardly all over the heavens down to the arc of a circle whose plane was inclined to the horizon about 10°. At length the light assumed a more intense form and shot up in whitish coruscations to the apex of the illuminated portion which was about 20° south of the zenith. My assistant observed this aurora at 10 P. M., and as it disappeared about midnight it must have lasted about two hours.

13. *Observations at Great Salt Lake City, Utah*, (lat. 40° 45′, long. 111° 26′), *by* W. W. PHELPS.

An aurora was seen here on the evening of Sept. 1st and morning of the 2d. A little after midnight, it spread from north to south, from east to west; and by its light I could tell the time on my watch. There was a beautiful center equally rayed near the zenith. At times the southern hemisphere began with pale red at the zenith, and faded down to a dark orange horizon, while the northern hemisphere glowed with yellow and green. I continued my observations till past 2 A. M.

Sept. 3d, about 8 P. M. a faint light sprung up in the east, and rose about 45° high. At 9^h 15^m it glowed beautifully. After 10 P. M. it grew fainter, and disappeared a little after midnight.

14. *Observations at New York City,* (lat. 40° 43′, long. 74° 5′), *by* Prof. O. W. Morris.

Aurora from 10^h 20^m P. M. Sept. 1st, till dawn of the 2d. There was a dark band at the south of 10° or 15°, then a white one and streamers of a variety of colors, mostly red, shot up in the S.E. and yellow in the S.W. interspersed with white beams. The corona was after 1 A. M. a little S. E. of the zenith.

Sept. 2d aurora from 10 P. M. to 11^h 15^m P. M. At first a faint light, then a dark segment above it on the northern horizon of about 15° in breadth; above it a band of about 8°; then another dark band, surmounted by a white one from which beams of bright light shot up along the upper edge. About 10^h 15^m waves of white light shot up nearly to the zenith. It faded away gradually after 11 P. M. retaining only a steady light on the horizon.

15. *Observations at Carlisle, Pennsylvania,* (lat. 40° 12′, long. 77° 11′), *by* Prof. C. W. Wilson.

Very brilliant aurora lasting from midnight to 3 A. M. Sept. 2d. Also Sept. 2d, 9 P. M. It first appeared as a luminous arch extending from N.W. to N.E. and 10° or 20° high. From this the light shot up in streamers first white, then turning bright red about half way to the zenith. The arch finally disappeared and, the whole then presented the appearance of a mass of light clouds, with rapid flashes behind them. The whole lasted about half an hour.

16. *Observations at Urbana, Ohio,* (lat. 40° 6′, long. 83° 11′), *by* Prof. M. G. Williams.

Sept. 2d, my first observation was at 1^h 30^m A. M. when the entire northern heavens were covered with a uniform yellowish light. There was also a large mass of crimson light in the S.W. At 2 A. M. the whole south was covered with pink and crimson light, soon after a distinct and beautiful corona was formed a few degrees south of the zenith, and continued about 15 minutes. At 2^h 40^m A. M. a beautiful column of light 12° in breadth white, yellowish, pink and crimson rose from the west at an angle of 70° with the southern horizon. At 3 A. M. the aurora was considerably diminished in intensity, but continued with variations till dawn of day.

Sept. 2d, at 8^h 45^m P. M. the light was distinct and uniform along the northern horizon. At 9^h 15^m there were fine coruscations increasing in brilliancy till 10^h. At 9^h 30^m the coruscations had for their base a well defined arch extending from N.

80° E. to N. 85° W., and its altitude at its center was 12°. Many of them reached the zenith but no corona was formed. The coruscations were mostly white.

Sept. 3d. The evening was cloudy, but the aurora was sufficiently strong to illuminate the clouds.

17. *Observations at Newark, Ohio*, (lat. 40° 4′, long. 82° 23′) *by* DAVID WYRICK.

Sept. 1st, about midnight the entire heavens except near the southern horizon were illuminated by a pale yellow light. In about ten minutes masses of red light appeared in the E. and W; and as they faded away others appeared in the N. and S. Subsequently there was a beautiful emanation of red rays from a circular center near the zenith. The whole phenomenon lasted about two and a half hours. In the S. and S.W. lay a dark cloud rising about five degrees above the horizon.

18. *Observations at Baltimore, Maryland*, (lat. 39° 18′ long. 76° 37′), *by* C. WESTBROOK, *Telegraph Superintendent.*

On the morning of Sept. 2d, I found the telegraph wires charged to an extent far beyond the strength of our ordinary batteries. Upon disconnecting the batteries I got clear and distinct writing from Cumberland, distant 179 miles. When the current was at its maximum strength, the manipulations of the operator at Cumberland worked the armature of the relay magnet here with a force nearly equal to that which would be produced by a Grove battery of 50 cups on a short circuit. The intensity of the spark at the instant of breaking the circuit, was such as to set on fire the wood work of the switch board. The current however was variable, and at times no sensible effect could be observed.

19. *Observations at Aurora, Indiana*, (lat. 39° 4′, long. 84° 54′), *by* GEORGE SUTTON, M.D.

Sept. 1st, about 10 P. M. a faint aurora was seen in the north; and about midnight the aurora extended over the whole heavens. In the north the light was of a pale color resembling the break of day, and a few faint streamers could be seen. About 1 A. M. the whole southern heavens presented a deep red appearance. At $1^h\ 30^m$ streamers were more frequently seen in the north, and occasionally a ray would appear in the S.E and S.W. Between 2 and 3 A. M streamers arose in all directions, but much paler in the north than in the south. The streamers converged to a point presenting the appearance of a vast and gorgeous tent. From 1 A. M. until the break of day, the most brilliant display was in the south.

Sept. 2d, about 8½ P. M. the aurora appeared again in the north. There were occasional flashes of light resembling distant lightning. It disappeared in a few hours.

20. *Observations at Auburn, California*, (lat. 38° 53′, long. 121° 2′), *by* ROBERT GORDON.

Sept. 1st, from 9½ P. M. until daylight we had a most magnificent display of aurora, in which the whole sky north, south, east and west was almost all the night glowing with ruddy light. The northern point near the horizon where the aurora commenced continued rather dark.

21. *Observations at St. Louis, Missouri*, (lat. 38° 37′, long. 90° 15′), *by* A. FENDLER.

Sept. 2d, at $8^h 20^m$ P. M. the aurora exhibited a white hazy light 15° above the northern horizon. Soon long white streaks appeared alternately on the right and left to 45° above the horizon, and a light red tint was sometimes visible. At $8^h 30^m$ the aurora disappeared. At $9^h 20^m$ P. M. it again threw out several white streaks to about 60° above the northern horizon. Presently it changed to a few fiery red spots. At $9^h 27^m$ a few short streaks shot up a little to the east of north. At $9^h 32^m$ the aurora became brilliant. At $9^h 34^m$ red with no streaks. Soon after nothing remained but white diffused light.

22. *Observations at Moneka, Kansas*, (lat. 38° 30′, long. 98°), *by* L. CELESTIA WATTLES.

Sept. 2d, an aurora appeared from 1 to 3 A. M. On the same night about 10 P. M. a light appeared in the S.E. like the rising of the moon. It grew redder and more brilliant as it extended up the sky until it reached the zenith. It now shot out to the westward, streaming continually across the sky to the horizon. It did not wholly disappear until the morning dawn.

23. *Observations at New Albany, Indiana*, (lat. 38° 17′, long. 85° 45′), *by* ALEXANDER MARVIN.

Sept. 2d, at 1^h 30^m A. M. a broad beam of crimson light extended up from the eastern horizon and met another from the west considerably south of the zenith. Beautiful streamers shot up all around the north to the point of meeting of the beams, south of the zenith. In the north a dark cloud extended from E. to W. in the form of a flat arch. Above it was an ever changing display of white and deep crimson streamers. A similar arch of cloud was visible in the south but not quite so flat; and above it, streamers concentrated in the point above mentioned. The greatest observed brilliancy was from $1^h 50^m$ to $2^h 30^m$ A. M.

Sept. 3d, $8^h 45^m$ P. M. streamers of white and pale red light shot from the horizon half way to the zenith. At 9 P. M. only a grey light in the north.

24. *Observations at Louisville, Kentucky*, (lat. 38° 3′, long. 85° 30′), *from the Louisville Journal.*

On the morning of Sept. 2d for some three hours commencing about midnight, the whole heavens were lighted up in the most brilliant manner. The light was generally diffused over the whole sky, but was reddest in a southwest direction. Towards the north it was whiter with occasional streaks of green and deep crimson darting up towards the zenith.

25. *Observations at the base of the Sierra Abajo, Utah*, (lat. 37° long. 110°, *by* Dr. John S. Newberry.

Sept. 1st, no aurora was observed at 9 P. M. the heavens being partially obscured by broken clouds. At 1 A. M. I waked and was startled by the red light that penetrated the tent and tinged the landscape, which was illuminated as strongly as by the full moon shining through thin clouds. On going out I found the whole heavens of a bright crimson with streaks of white and yellow converging toward the zenith, where they formed a beautiful corona. These rays reached down within 20° of the southern horizon. The aurora continued almost without abatement till day-light.

26. *Observations at Bentonville, Arkansas*, (lat 36° 22′, long 94° 16′), *by* Paul Graham.

Sept. 1st, at 11 P. M. there was a bright sheet of light embracing the eastern and northeastern part of the horizon, from thence extending upwards 60°. It became so light that objects were clearly seen at a considerable distance. At the same time, the clouds in the S. W. were tinged with red. It continued very bright until 3 A. M. at which time it disappeared.

27. *Observations at Asheville, N. Carolina*, (lat. 35° 37′, long. 82° 29′), *by* H. H. Strawbridge.

Sept. 2d, at $12^h 30^m$ A. M. my attention was attracted to a singular fiery light in the N.E. at an elevation of about 45°. Some 3° or 4° to the east of north also apppeared a space of whitish light. Both lights increased, the spots of faint reddish light extending quite to the zenith. At $12^h 50^m$ the lights diffused themselves over the sky, and from about N.E. to S.W. a complete belt of roseate light extended from horizon to horizon across the zenith. It was at first about 6° or 8° in breadth, its edges very ill-defined; but it gradually widened and changed its line of direction until it became a zone of rosy light from 25° to 40° in breadth, reaching from the west to a little north of east. From $1^h 5^m$ to $1^h 25^m$ A. M. I was able to read with perfect ease the smallest type in a newspaper. At $1^h 30^m$ the light slightly decreased. At 2 A. M. a large space in the belt about E.S.E. and 50° above the horizon became more intense. From $2^h 30^m$ the clouds so thickened as to prevent observations, although the red zone was still clearly traceable.

28. *Observations at Memphis, Tennessee*, (lat. 35° 8′, long. 90° 0′), *by* R. W. MITCHELL, M.D.

Sept. 1st, at midnight a splendid aurora flamed up suddenly in the north. Its breadth was about 30°, extending 15° on each side of the meridian and its altitude was about 40°. At first its color was nearly blood red, and motionless; but about $12^h 30^m$ A. M. streamers began to appear though with little or no change of color. In less than an hour the deep red began to fade, and it continued thus until 4 A. M. when it vanished.

29. *Observations at Selma, Alabama*, (lat. 32° 25′, long. 86° 51′), *by* S. K. JENNINGS, M.D.

Sept. 2d, about 12½ A. M. a strip of red cloud nearly transparent and 9° or 12° wide commenced in the east, and soon extended across to the west, forming a magnificent arch. It was striped with the various hues of red from light to brightest scarlet, with a tinge of straw color, and from the centre of the arch diverging rays looking to the south and reaching nearly to the horizon. The rays colored like the arch were soon scattered, but the main arch did not entirely disappear until 4¾ A. M.

30. *Observations at Paulding, Mississippi*, (lat. 32° 20′, long. 89° 20′), *by* Rev. E. S. ROBINSON.

Sept. 2d, at $2^h 10^m$ A. M. nearly the whole visible heavens were overspread with a gauze-like lurid tint which continued till $3^h 30^m$ A. M. It was most brilliant in the N. E. and N. W. and at 2^h 30^m extended thirty degrees south of the zenith.

31. *Observations at Cahawba, Alabama*, (lat. 32° 19′, long. 87° 16′), *by* Dr. MATTHEW TROY.

Sept. 2d, the aurora was first observed about 1 A. M. An arch spanned the heavens from E. to W. a few degrees south of the zenith. To the north the sky had a distinct greenish tinge. The most magnificent displays of colored light were nearly over head. The light was so great that fine newspaper print could be read by it; and it continued with varying brilliancy till obscured by daylight.

32. *Observations at Natchez, Mississippi*, (lat. 31° 34′, long. 91° 25′), *by* J. J. SCOTT.

Sept. 1st, the aurora commenced at 10 P. M. I began to observe it at $12^h 15^m$ A. M., Sept. 2d. A glowing arch of the deepest crimson and 8° or 10° in breadth, extending from the N.E. to the N. W. points of the horizon, rising to the height of about 40°, while a fainter arch appeared below it. At 12^h 30^m rays emanated in all directions except south of a great circle passing through a point situated in the wing of Pegasus. From this point issued a broad flare of light which waved like a pennant. Every

where in the northern sky, patches of light would appear, glow for a time and gradually disappear. These appearances continued throughout the night, growing fainter as the dawn approached.

33. *Observations at Wheelock, Texas,* (lat. 30° 55′, long. 87° 29′), *by* F. KELLOGG.

At 10 P. M. Sept. 1st, I first observed a zone of crimson light 30° in breadth, reaching from 10° above the horizon due east, vertically overhead, and terminating 10° above the horizon due west. From the zenith to the eastern extremity of the zone the light was mild, the color increasing in intensity toward the east, until within 15° of the horizon where it gradually faded. At $11^h 20^m$ a beam of whitish light passed due north through the zenith. At $11^h 30^m$ another beam diverged from the former to the west, making an angle with it of 40°. These two beams, if continued, would unite about 25° south of the zenith. At midnight the entire space between these beams was filled up with similar but shorter beams of light, converging toward each other. Soon these central rays began to shoot bright scintillating rays of white light from their northern and western ends which travelled with great velocity. The eastern boundary of the zone became gradually paler until $12^h 30^m$ A. M. Sept. 2d, when the color in that direction entirely disappeared, and the brightest light was then in the west and northwest. At 1 A. M. the crimson color had entirely disappeared, and nothing remained but the fan-like appearance of the numerous divergent beams of white light. The two rays first formed never changed their form or position until they disappeared about 2 A. M.

34. *Observations at Thomasville, Georgia,* (lat. 30° 50′, long. 84° 0′), *by* W. BLAVETT.

Sept. 2d, about 2 A. M. the whole northern half of the heavens was beautifully illuminated. The daily track which the sun now describes formed the southern boundary of the illuminated portion of the heavens. Upon this southern boundary was a border of deep blood red light, of 2° or 3° in breadth, extending from the eastern to the western horizon. Streams of pale light diverged from the point where this band cut the meridian. These pale streamers, at one moment, were numerous, and the next moment scarce a trace of them could be seen. The great red belt sometimes changed to a beautiful orange color.

35. *Observations at Mobile, Alabama,* (lat, 30° 41′, 88° 1′), *from the Daily Mercury.*

Sept. 2d, the aurora appeared at midnight and soon after 1 A. M. the eastern sky seemed bathed in a flame of lurid light, while a yet deeper flame streaked with silvery beams the N.W. These two pillars were united in the zenith, by a broad belt of

dimmer fire. The pyramidal foci of red light were situated, one in the east, the other in the N.W. A little later the red field in the N.W. extended southerly so that one half the western sky from the horizon to the zenith seemed a blaze of fire. Meantime brilliant streamers continued to shoot from the N.E. and N.W. towards the zenith, sometimes extending over 50° or 60° of the heavens. These streamers converged towards a point on the meridian about 15° south of the zenith, and from this point shot forth smaller pencils of silvery showers. At $3^h\ 30^m$ the play of the streamers had ceased, while the flash of fiery red had spread over the whole north. The red flush in the northern quarter of the heavens, continued to glow until obscured by the solar dawn.

36. *Observations at Washington, Texas*, (lat. 30° 26′, long. 96° 15′), *by* Maj. B. F. RUCKER.

Sept. 1st, at $10^h\ 30^m$ P. M. I observed a bright light in the north and N.E. At $11^h\ 30^m$ the light had become much stronger and a good deal more extended in the base; and some beautiful rays shot far up on the sky in the north. At midnight the base of fiery looking vapor extended from N.E. to N.W. The rays of fiery colored light rose from every direction like an inverted fan and converged towards a point several degrees south of the zenith. Some of these rays appeared like immense columns; others only as a thin streak; some were pale, others fiery. At 1 A. M. the light continued undiminished, although the darting rays were not so numerous. The aurora continued until obscured by the light of the sun.

37. *Observations at Jacksonville, Florida*, (lat. 30° 15′, long. 82°), *by* Dr. A. J. BALDWIN.

Sept. 2d, the aurora was witnessed from midnight till daylight. At 3 A. M. the entire heavens, even at the extreme south were in a red glow. Streamers ran up from a point in the N.W. and from the S.E. and tortuous waves swelled up from the bottom of these streamers and illuminated the whole heavens. At times these looked like lambent flames, flickering like a blaze of fire.

38. *Observations at Union Hill, Texas*, (lat. 30° 11′, long. 96° 31′), *by* Dr. WM. H. GANTT.

Sept. 1st, at 11 P. M. a faint glimmering light was visible in the N.E., which gradually grew brighter and extended over a larger space. At midnight it reached from the north 35° eastward, and mounted nearly to the zenith, and soon began to be seen west of north. At 1 A. M. it extended from west to east, and beyond the zenith. Towards the north, extending east and west about 20°, and rising about 10° above the horizon was a dark looking cloud. Above this, the light was of a whitish color, and

from it sprang streamers of pink merging into crimson. The grandest display was from 1 A. M. to 1^h 35^m. It now began to fade, and at 3^h 30^m was nearly gone. A few flashes of it, however, remained until daylight.

39. *Observations at Micanopy, Florida*, (lat. 29° 30′, long. 82° 18′), *by* JAMES B. BEAN.

Sept. 2d, at 12^h 30^m A. M. I first saw a luminous haze in the north, and at 12^h 35^m streamers shot up in the north. A few minutes before 1 o'clock a luminous arch appeared, but not well defined. At 1 A. M. it included 160° of the horizon; at 1^h 10^m there were many distinct streamers; at 1^h 15^m beautiful quivering streamers, while patches of white light appeared in different parts of the northern hemisphere. At 1^h 25^m the corona was very bright; at 1^h 35^m corona very distinct, of vivid white clouds of light. At 1^h 40^m very brilliant red beams in the west; at 1^h 50^m the arch extended from E. to W. passing through Aries and Pegasus; at 2^h a faint corona; at 2^h 11^m distinct beams near the zenith and on each side E. and W.; at 2^h 25^m arch brighter red with red patches of light and distinct streamers reaching beyond the zenith; at 2^h 35^m arch fading, at 2^h 40^m red light in N.W. but streamers not so distinct; at 2^h 50^m beautiful beams in N.E.; at 3^h arch disappearing, beams indistinct; at 3^h 10^m red haze and no beams; at 3^h 30^m very faint red haze, and faint white light near the horizon.

40. *Observations at Corpus Christi, Texas*, (lat. 27° 45′, long. 97° 30′), *by* A. M. LEA.

Sept. 1st, the aurora began about $11\frac{1}{2}$ P. M, and continued until daylight. Two-thirds of the whole visible heavens were lighted up with a rich red glow, whilst the tremulous columns of variegated light swept over the heavens, from the northern horizon through the zenith to a line within 40° of the horizon on the south. Its greatest intensity was about $1\frac{1}{2}$ A. M. Sept. 2d.

41. *Observations at Fort Jefferson, Florida*, (lat. 24° 37′, long. 82° 52′), *by* Capt. D. P. WOODBURY.

Sept. 2d, at 12^h 45^m A. M. a continuous arch of red color extended from N. 50° E. to N. 50° W. having an altitude of about 15°, and the thickness of the arch throughout was about 25°. The shade of red was deepest along the central part of the arch, gradually diminishing above and below. Soon rays began to appear in faint white lines; they grew brighter, and extended above and sometimes a little below the arch. Soon the rays became numerous, traversing the red arch in right lines, and converging to a point in the magnetic meridian somewhat south of the zenith. They sometimes extended as high as the zenith, and even beyond. The aurora continued, gradually fading, till day light.

42. *Observations at Key West, Florida,* (lat. 24° 33′, long. 81° 48′), *by* WILLIAM C. DENNIS.

Sept. 2d, at $2\frac{1}{2}$ A. M. there were two patches of brilliant ruddy lights one in the N.E. and the other in the N.W. From the North extending 15° toward the E. there were rays of light shooting toward the zenith, the longest reaching full 60°. At $2^h\ 50^m$ the patch of light in the N.E. broke up into most brilliant rays extending toward the zenith. At 3 A. M. the patch of light in the N.W. also broke up into rays of light in a similar manner. At $3^h\ 15^m$ the rays gradually disappeared, but there still remained brilliant ruddy lights in the N.E. and N.W. At $3\frac{1}{2}$ A. M. the aurora was decidedly fainter, and at 4 A. M. nothing but a faint glow remained. This glow did not entirely disappear until overcome by the light of day.

43. *Observations at Sea,* (lat. 12° 23′ N., long. 88° 28′ W.), *by* Commander W. D. PORTER, U. S. Navy.

Sept. 1st, about $11\frac{1}{2}$ P. M. the sky had a lurid appearance in the north, and there were occasional flashes of lightning. The rest of the sky was clear, bright and beautifully blue. The red appearance was very much like the aurora of high latitudes, and now and then it had a wavy appearance. About 1 A. M. a body of heavy clouds passed over with rain.

ART. XXXII.—*On the direction of molecular motions in Plane Polarized Light;* by Prof. W. H. C. BARTLETT.

Fresnel's Formulas for reflexion and ordinary refraction.

IT is proposed, in the following paper, to deduce these formulas from the principle of the conservation of living force and the rules for the resolution and comparison of forces, without any hypothesis in regard to the condition of the ether in the co-terminous bodies. If this were the only object, however, the solution would hardly be worth the space asked for it in the Journal, because all now-a-days believe in the truth of these formulas. But there is a purpose beyond this. The mode of solution may settle the vexed question in regard to the direction of the molecular motions in plane polarized light; and, on this account, it will have an interest for those who are still in doubt upon the subject. There are few opinions among scientific men held more loosely than that which relates to this question. Some contend for vibrations in, and others perpendicular to, the plane of polarization; and a few of the most eminent and thorough investigators have entertained both opinions at different times,

having changed not once but twice or thrice. Although Professor McCullogh has shown, in his elaborate treatise on crystalline reflexion and refraction, as published in the memoirs of the Royal Irish Academy, vol. xviii, that vibrations in the plane of polarization are absolutely essential to the theory, still other results are adduced which lean to the opposite view, and opinions are still divided.

When a wave of a given length is plane polarized, by a single reflexion, the plane which contains the incident and reflected rays, is called *the plane of polarization.* Are the molecular vibrations, which in this case are rectilinear, performed in this plane or at right angles to it?

Take the well known wave function, say the first of Equations (528), Bartlett's Analytical Mechanics, omitting the subscripts, x,

$$\xi = \alpha . \sin \frac{2\pi}{\lambda} (Vt - r) \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

in which the molecular vibrations are parallel to the axis x, and the wave propagation in the direction of the plane yz, ξ the actual molecular displacement at the time t, α its maximum value, V, the velocity of wave propagation, λ the wave length, and r the distance of the molecules place of rest from the origin.

Living force and quantity of motion in a plane polarized wave.

Differentiate eq. (1), with respect to ξ and t, we find:—

$$\frac{d\xi}{dt} = \alpha . V . \cos \frac{2\pi}{\lambda} (Vt - r) \frac{2\pi}{\lambda}.$$

Denote the density of the medium by Δ, and the area of any portion of the wave-front by a, then will the mass between two consecutive positions of this area be $a . \Delta . dr$, and the living force within a quarter of a wave-length be:—

$$\left.\begin{aligned} &\int_{r+\frac{1}{4}\lambda}^{r} \Delta . a . dr \frac{d\xi^2}{dt^2} = \int_{Vt-r=\frac{1}{4}\lambda}^{Vt-r=0} \Delta . a . \frac{2\pi}{\lambda} . \alpha^2 . V^2 . \cos^2 \frac{2\pi}{\lambda} (Vt - r) \\ &\times \frac{2\pi}{\lambda} dr = \frac{1}{2} \frac{\pi^2}{\lambda} . V . \Delta . a . V . \alpha^2. \end{aligned}\right\} \quad (2.)$$

Dividing by the volume $a . V$, and recalling that π and $\frac{V}{\lambda}$ are constant, we shall find that the quantity of living force in a unit of volume of the medium will vary directly as the product of the density and square of the greatest displacement; and the relation of these products, in the case of any two waves, will determine the relation of the effects of these waves upon the organs of sense upon which they act.

Again, the quantity of motion in this quarter of wave-length will be:—

$$\int_{r+\frac{1}{4}\lambda}^{r} \Delta.a.dr.\frac{d\xi}{dt} = \int_{Vt-r=\frac{1}{4}\lambda}^{Vt-r=0} \Delta.a.\alpha.\,V.\cos\frac{2\pi}{\lambda}(Vt-r)\frac{2\pi}{\lambda}dr = \Delta.a.\alpha.\,V. \quad (3.)$$

Resolution of living force and of motion, by deviating surfaces.

Take the co-ordinate plane $x\,z$ in the plane of incidence, and the axis z in the direction of the normal to the incident wave, the axis y will be parallel to the line of the nodes of the molecular orbit in the deviating surface, at the place of incidence. Then denoting any portion of the trace of the plane of incidence on the denoting surface by s, and the angle of incidence and of reflection by φ and φ', respectively, will the element of the deviating surface at the place of incidence be $ds.dy$, and its projections upon the incident, reflected, and refracted wave-fronts, respectively, be $ds.dy.\cos\varphi$, $ds.dy\cos\varphi$, and $ds.dy.\cos\varphi'$. These will take the place of a in Equations (2) and (3), in computing the living force and quantity of motion in the incident, reflected and refracted waves.

Now take a wave of common light and replace it by its two components, having the vibrations in the one parallel and in the other perpendicular to the plane of incidence.

First take the component wave in which the molecular motions are parallel to the plane of incidence, and therefore parallel to the axis x, and employ the subscripts i, r and t, to denote the incident reflected and transmitted or refracted component waves, respectively. The living force in the incident, must be equal to the sum of the living forces in the reflected and refracted components; whence, equation (2), omitting the common factors,

$$\Delta.\cos\varphi.\,V.\alpha^2{}_{xr} + \Delta_{,}.\cos\varphi'.\,V_{,}.\alpha^2{}_{xt} - \Delta.\cos\varphi.\,V.\alpha^2{}_{xi} = 0\,;$$

or, recalling that $\frac{V}{V_{,}} = \frac{\sin\varphi}{\sin\varphi_{,}}$,

$$\alpha^2{}_{xr} + \frac{\Delta_{,}}{\Delta}.\frac{\cos\varphi'}{\cos\varphi}.\frac{\sin\varphi'}{\sin\varphi}.\alpha^2{}_{xt} - \alpha^2{}_{xi} = 0 \quad . \quad . \quad . \quad (4.)$$

in which Δ and $\Delta_{,}$ are the densities of the medium of incidence and of intromittance.

The molecular motions are all parallel to the plane of incidence, and at the same time normal to the directions of their respective wave motions; they, therefore, make with one another angles equal to those made by the directions of these latter motions, and we obtain two more equations from the rules for the resolution and composition of oblique forces. The angles made by the direction of the motion in the incident with the directions of the motions in the reflected and refracted waves, are $180°-2\varphi$ and $360°-(\varphi-\varphi')$, respectively; and the angles under which the

directions of the motions in the latter waves are inclined to one another, is $180^\circ-(\varphi+\varphi')$. Whence:—

$$\Delta.\cos\varphi.\ V.\alpha_{xr}=-\Delta.\cos\varphi.\ V.\alpha_{xi}.\frac{\sin(\varphi-\varphi')}{\sin(\varphi+\varphi')};$$

$$\Delta_{,}.\cos\varphi'.\ V_{,}.\alpha_{xt}=\Delta.\cos\varphi.\ V.\alpha_{xi}.\frac{\sin 2\varphi}{\sin(\varphi+\varphi')};$$

or,

$$\alpha_{xr}=-\alpha_{xi}.\frac{\sin(\varphi-\varphi')}{\sin(\varphi+\varphi')}\quad\ldots\ldots\ldots\quad(5.)$$

$$\alpha_{xt}=\alpha_{xi}.\frac{\Delta}{\Delta_{,}}.\frac{\cos\varphi}{\cos\varphi'}.\frac{\sin\varphi}{\sin\varphi'}.\frac{\sin 2\varphi}{\sin(\varphi+\varphi')}\quad\ldots\ldots\quad(6.)$$

Substituting these in equation (4.), we readily find:—

$$\frac{\Delta}{\Delta_{,}}=\frac{4\cos^2\varphi'.\sin^2\varphi'}{\sin^2 2\varphi}=\frac{\cos^2\varphi'.\sin^2\varphi'}{\cos^2\varphi.\sin^2\varphi};$$

whence,

$$\sqrt{\Delta_{,}}=\sqrt{\Delta}.\frac{\sin 2\varphi}{2\cos\varphi'.\sin\varphi'}=\sqrt{\Delta}.\frac{\cos\varphi.\sin\varphi}{\cos\varphi'.\sin\varphi'}\quad\ldots\quad(7.)$$

Substituting the above ratio of the densities in the equation just preceding, we get:—

$$\alpha_{xt}=\alpha_{xi}.\frac{2\cos\varphi'.\sin\varphi'}{\sin(\varphi+\varphi')};\quad\ldots\ldots\ldots\quad(8.)$$

multiplying this by equation (7), member by member, and the equation giving the value of α_{xr}, by $\sqrt{\Delta}$, and taking:—

$$\sqrt{\Delta}.\alpha_{xi}=1;\quad\sqrt{\Delta}\alpha_{xr}=v;\quad\sqrt{\Delta_{,}}.\alpha_{xt}=u,$$

we find:—

$$v=-\frac{\sin(\varphi-\varphi')}{\sin(\varphi+\varphi')}\quad\ldots\ldots\ldots\quad(9.)$$

$$u=\frac{\sin 2\varphi}{\sin(\varphi+\varphi')}\quad\ldots\ldots\ldots\quad(10.)$$

To which may be added the relations,

$$\sin\varphi'=\frac{\sin\varphi}{m};\quad\cos\varphi'=\sqrt{1-\frac{\sin^2\varphi}{m^2}}.$$

Transposing the term of which α_{xt} is a factor to the second member in equation (4), subtracting equation (5) from $\alpha_{xi}=\alpha_{xi}$, dividing the first result by the second, and multiplying the quotient by equation (5), we readily find:—

$$\frac{\alpha_{xi}+\alpha_{xr}}{\cos\varphi}=\frac{\alpha_{xt}}{\cos\varphi'}\quad\ldots\ldots\ldots\quad(11.)$$

That is, the projection in the direction of wave propagation and on the deviating surface, of the greatest displacement in the incident, increased by that in the reflected wave, is equal to like projection of the greatest displacement in the refracted wave.

Next, take the wave in which the molecular motions are perpendicular to the plane of incidence and therefore parallel to the axis y; these are parallel to the deviating surface. The motions in the incident, reflected and refracted waves are parallel to one another, and, by the principles of parallel forces, the sum of the motions in the reflected and refracted waves must be equal to that in the incident. The equation for the living force will be the same as before. Whence:—

$$\Delta . \cos\varphi . V . \alpha^2{}_{yr} + \Delta_{,} . \cos\varphi' . V_{,} . \alpha^2{}_{yt} - \Delta . \cos\varphi . V . \alpha^2{}_{yi} = 0;$$

$$\Delta . \cos\varphi . V . \alpha_{yr} + \Delta_{,} . \cos\varphi' . V_{,} . \alpha_{yt} - \Delta . \cos\varphi . V . \alpha_{yi} = 0. \quad (12.)$$

In which Δ and $\Delta_{,}$ are, as before, the densities of the medium of incidence and of intromittance, respectively; or,

$$\alpha^2{}_{yr} + \frac{\Delta_{,}}{\Delta} . \frac{\sin\varphi'}{\sin\varphi} . \frac{\cos\varphi'}{\cos\varphi} . \alpha^2{}_{yt} - \alpha^2{}_{yi} = 0$$

$$\alpha_{yr} + \frac{\Delta'}{\Delta} . \frac{\sin\varphi'}{\sin\varphi} . \frac{\cos\varphi'}{\cos\varphi} . \alpha_{yt} - \alpha_{yi} = 0. \quad . \quad . \quad . \quad (13.)$$

Transposing the terms containing α_{yr} and α_{yi} to the second members, and dividing the first by the second, we find:—

$$\alpha_{yr} + \alpha_{yi} = \alpha_{yt}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14.)$$

That is, the greatest displacement in the refracted is equal to the sum of the greatest displacements in the incident and reflected waves.

Substituting the value of $\frac{\Delta'}{\Delta}$, as given by equation (7), in equation (13), we have:—

$$\alpha_{yr} + \frac{\sin\varphi . \cos\varphi}{\sin\varphi' . \cos\varphi'} . \alpha_{yt} - \alpha_{yi} = 0. \quad . \quad . \quad . \quad . \quad (15.)$$

Substituting in this, first the value of α_{yt}, and then of α_{yr}, deduced from equation (14), we readily get:—

$$\alpha_{yr} = -\alpha_{yi} . \frac{\tan(\varphi - \varphi')}{\tan(\varphi + \varphi')}; \quad . \quad . \quad . \quad . \quad . \quad (16.)$$

$$\alpha_{yt} = \alpha_{yi} . \frac{4\cos\varphi' . \sin\varphi'}{\sin 2\varphi + \sin 2\varphi'}. \quad . \quad . \quad . \quad . \quad . \quad (17.)$$

Multiplying the first of these by $\sqrt{\Delta}$, and the second by equation (7), and making:—

$$\sqrt{\Delta} . \alpha_{yi} = 1; \quad \sqrt{\Delta} . \alpha_{yr} = v'; \quad \sqrt{\Delta_{,}} . \alpha_{yt} = u';$$

there will result,

$$v' = -\frac{\tan(\varphi - \varphi')}{\tan(\varphi + \varphi')} \quad . \quad . \quad . \quad . \quad . \quad . \quad (18.)$$

$$u' = \frac{\sin 2\varphi}{\sin(\varphi + \varphi') . \cos(\varphi - \varphi')}. \quad . \quad . \quad . \quad . \quad . \quad (19.)$$

18. Divide equation (10) by equation (9), and equation (19) by equation (18), replace v, u, v' and u' by their values, and substitute for the ratio of the square roots of the densities its value as given in equation (7), we find:—

$$\frac{\alpha_{xt}.\cos\varphi'}{\alpha_{xr}.\cos\varphi}=-\frac{\cos\varphi'}{\cos\varphi}.\frac{\sin 2\varphi'}{\sin(\varphi-\varphi')},$$

$$\frac{\alpha_{yt}}{\alpha_{yr}}=-\frac{\sin 2\varphi'}{\sin(\varphi-\varphi').\cos(\varphi+\varphi')}. \quad . \quad . \quad . \quad . \qquad (20.)$$

But $\alpha_{xt}.\cos\varphi'$ and $\alpha_{xr}.\cos\varphi$, are the components parallel to the deviating surface of the displacements which are in the plane of incidence; α_{yt} and α_{yr} are already parallel to the deviating surface; whence, as long as $\varphi>\varphi'$, that is as long as the velocity of wave-motion in the medium of incidence exceeds that in the medium of intromittance, the molecular phases in the refracted and reflected waves will be opposite, and conversely.

Denote the living force in the incident wave of common light by unity, that in each of its component waves will be denoted by half of unity; and the total living force in the reflected components will, eqs. (9) and (18), be:—

$$v^2+v'^2=\tfrac{1}{2}.\frac{\sin^2(\varphi-\varphi')}{\sin^2(\varphi+\varphi')}+\tfrac{1}{2}.\frac{\tan^2(\varphi-\varphi')}{\tan^2(\varphi+\varphi')} \quad . \quad . \qquad (21.)$$

and in the refracted components, eqs. (10) and (19),

$$u^2+u'^2=\tfrac{1}{2}.\frac{\sin^2 2\varphi}{\sin^2(\varphi+\varphi')}+\tfrac{1}{2}.\frac{\sin^2 2\varphi}{\sin^2(\varphi+\varphi').\sin^2(\varphi-\varphi')}. \qquad (22.)$$

If $\varphi+\varphi'=90°$, then will:—

$$\sin\varphi=m\sin\varphi'=m\cos\varphi,$$

or,

$$\tan\varphi=m;$$

in which φ is the maximum polarizing angle; the wave will therefore be wholly polarized; and since eq. (21) will reduce, in the second member, to the first term, the wave will contain those vibrations only which are parallel to the plane of incidence; that is, the vibrations will be in the plane of polarization. The intensities of the reflected and transmitted components will be, respectively, under this condition,

$$\tfrac{1}{2}\cos^2 2\varphi \text{ and } \tfrac{1}{2}\sin^2 2\varphi+\tfrac{1}{2}\tan^2 2\varphi.$$

West Point, N. Y., 1860.

ART. XXXIII.—*On some Questions concerning the Coal Formations of North America;* by LEO LESQUEREUX.

(Continued from page 74.)

Stratigraphical distribution of the Coal-flora.

DETAILS concerning the stratigraphical distribution of the coal-flora of North America, to be intelligible, ought to be prefaced by a few remarks on the order of superposition of the strata of the coal-measures.

A general section, showing the order of stratification in the whole extent of our coal-fields, would appear perhaps merely hypothetical. Such is, nevertheless, the uniformity of the distribution of the strata of our coal-basins, that a section made in Western Illinois or Western Kentucky or in any part of the coal-fields of these States, will prove comparatively similar, (that is with some difference in the thickness of the strata,) to any section made in the coal-fields of Pennsylvania or of Ohio. This analogy of stratification has been fully established by a series of comparative sections, reported for the 4th vol. of the State Geological Survey of Kentucky. Such a comparison of sections had been attempted before, for the State of Pennsylvania, by my friend J. P. Lesley, in his excellent Manual of Coal. But it was not based on palæontological evidence and thus the contemporaneousness of the juxtaposed strata was necessarily problematical. On the contrary, by admitting the similarity of the flora of the coal-strata as a coëval mark and as a basis for juxtaposition of the strata, the result of the comparison of sections gives evidence remarkable in a double point of view. First it shows, by juxtaposition of the coal strata of which the shales contain the same species of fossil plants, the uniformity of stratification in the whole area of the coal-fields of the United States; and secondly it proves that the distribution of the coal plants has followed the same developement, the same successive modification in the whole extent of the same coal-field.

Though plants of carboniferous genera are found below the upper bed of the Archimedes or mountain limestone, as, apparently, no coal has been formed at this low station, I take as the base of the true coal measures this Archimedes limestone, replaced in Eastern Pennsylvania and Northern Virginia by the red shales of the old red sandstone. From the top of the mountain limestone, to the top of the millstone-grit series, including some beds of coal at its base, the average thickness is from three to four hundred feet. The upper part of the millstone-grit series, sometimes its whole thickness, is a compound of coarse sandstone, shales and especially conglomerate, the last predominat-

ing. The conglomerate formation thickens wonderfully at some places, generally increasing westward. Its greatest thickness, in the sharp Mountain near Pottsville, Penn., is 1100 feet.*

To elucidate the details of this general section, we can admit the numbered division of the coal strata, as it has been most satisfactorily established by Dr. D. Dale Owen for the State Geological Survey of Kentucky, vol. iii, p. 18. From the top of the millstone-grit to the base of another great sandstone formation, the Mahoning sandstone, there are generally five workable beds of coal. The lowest is No. 1A; then No. 1B to No. 4 inclusively. No. 1B coal is the Big, or Mammoth coal of the East. Like the conglomerate, it increases in thickness progressively westward. The fourth bed of coal is the Pomeroy coal of Ohio. The Mahoning standstone overlying No. 4 bed of coal, like the millstone-grit is generally conglomerate at its upper part. Its thickness averages from one hundred to one hundred and fifty feet. It might, as well as the true conglomerate, be considered as the basis of a new coal epoch; even moré so, perhaps, since the vegetation at its base and at its top shows a greater difference than in the coal strata above and below the millstone-grit. From the Mahoning sandstone upward, the marine influence predominates more and more and the shales of the coal strata more generally contain remains of fishes and of marine shells than of terrestrial plants. This Mahoning sandstone is separated by about 500 feet of strata from another great sandstone formation still resembling in its composition and thickness the Millstone-grit or conglomerate. It is the Anvil-rock sandstone of the State Geological Survey of Kentucky. The 500 feet of measures between it and the Mahoning, are sometimes barren of coal. But westward they contain from Coal No. 6 to No. 12, five workable beds of coal and two, scarcely if ever, of a workable thickness.† The highest coal strata of this section, Nos. 11 and 12 united together, form the great Pittsburg coal bed of Pennsylvania.

Above the Anvil-rock sandstone, there are still 500 to 700 feet of coal measures, in which some thin beds of coal are formed. But this examination of the flora of the coal measures can not be extended higher than the Anvil-rock. The upper division has been locally washed away by erosion and where it is still present, its coal beds are too thin for working and thus their unexposed shales can not be satisfactorily examined. Near Shawneetown, Illinois, the first coal above the Anvil-rock

* H. D. Rogers's final report of the State Geol. Survey of Pennsylvania, vol. i, p. 109.

† In those coal regions of the United States where coal is abundant, a bed of bituminous coal is not remunerative, when under a thickness of three and a half feet. In the Anthracite basin, the working becomes unprofitable for a bed of less than two and a half feet.

sandstone has a roof of laminated sandstone, blackened by broken and undeterminable leaves and small stems of ferns. The second bed at the same place contains only fossil shells and remains of fishes.

The first trace of a terrestrial vegetation in the Palæozoic strata of North America appears in the Marcellus shales or middle Devonian, in a species of *Lepidodendron* named *L. primævis* by Prof. H. D. Rogers.* It has not been described, but only obscurely figured by a wood-cut; and as I have not seen the specimen and could detect at the place where it has been found, near Huntingdon, only detached leaves of this species, its relation or its specific value is unknown to me. In the Devonian black shales of Ohio, and perhaps of Illinois,† large silicified trunks of trees have also been found, always very rare and at far distant localities.

Ascending higher, we find species of *Lepidodendron,* of *Sigillaria*, and especially of *Calamites* and stems (*Bornia* Sternb. and *Stemmatopteris* Göpp.) in the transition series of the Old Red sandstone of Pennsylvania, and especially in the Mountain limestone by which it is represented in the Western States. Leaves are very rare in this formation and of a different type from those of the coal measures. Heretofore, all those which have been found belong to the genus *Noeggerathia* (Sternb.), of which I have never seen a single specimen in the true coal measures.‡ Below the 3d or upper Archimedes limestone, there is in Illinois and in Arkansas a bed of schistose sandstone which, with peculiar species, already contains some others of the true coal measures. In the Cabinet of the State Geological Survey of Illinois, I have seen from this subcarboniferous sandstone, specimens of *Stigmaria anabathra* (Corda), and Göppert's varieties: *minor*, and *undulata, reticulata, stellata;* of *Lepidodendron Veltheimianum* Sternb., a species peculiar to the transition series, both in Europe and America; *of Lepidodendron Worthianum* Lsqx., a beautiful species related to *L. Volkmannianum* Sternb.; one *Lepidodendron* sp. nov.*;* one *Caulopteris*, one *Megaphytum*, all three new and undescribed species; *Calamites Suckowii* Brgt.; one *Bornia* Sternb.;

* Final report of the State Geol. Survey of Penn., vol. ii, p. 828.

† Mr. A. H. Worthen, State Geologist of Illinois has in his cabinet this Devonian silicified wood without label of locality. It is also in the cabinet of Dr. D. Dale Owen at New Harmony, also without label, All my specimens have been cut from a large tree protruding from Devonian strata near Delaware, Ohio, and discovered by Dr. Mann, who kindly communicated them to me. It is by all appearance the wood of a Lepidodendron, and it will be figured and described with the silicified Psaronius of the coal measures.

‡ Dr. Newberry mentions in his catalogue two species of Noeggerathia found in the coal above the conglomerate. One is *Noeggerathia Beinertiana* Göpp., a species evidently referable to *Cordaites* Ung., and probably only a small form of *Cordaites borassifolia* Ung. The other is *Noeggerathia microphylla* Newb., undescribed and consequently unknown to me.

Cordaites borassifolia Ung., *Knorria imbricata* Sternb., and some undeterminable stems. From these plants, six have been found also above the conglomerate series, ascending to coal No. 1B, or even higher. They are *Stigmaria anabathra*, var. *undulata* Göpp., and var. *minor* Göpp., *Lepidodendron Worthianum* Lsqx., *Knorria imbricata* Sternb., *Calamites Suckowii* Brgt. and *Cordaites borassifolia* Ung. These two last species are present in the whole thickness of the coal measures, as high at least as the 12th coal.

As I have said before, no coal has been seen as yet formed below the 3d Archimedes limestone. But just overlying it, a bed of coal is generally present over the whole extent of the Western coal-fields. In Arkansas, this is the only workable bed of coal, its thickness varying from 18 inches to 4½ feet. In the western part of the Eastern coal-basin of Kentucky, and also in Virginia, two, even three beds of good coal have been formed below the millstone grit. All the species of fossil plants of the shales of these coal strata have been found also in Arkansas. At Pottsville and Mauch Chunk, near the eastern margins of the coal-fields, the coal is formed between strata of conglomerate, and even at the base of this formation.

The shales of the subconglomerate coal contain not only remains of trees of large size, like the subcarboniferous sandstone, but thus early and simultaneously many of the species of ferns which become more and more abundant above the conglomerate series. Thus *Pecopteris velutina* Lsqx., *Neuropteris flexuosa* Brgt., *N. hirsuta* Lsqx., *Sphenophyllum Schlotheimii* Sternb., *Pecopteris nervosa* Brgt., *Annularia sphenophylloides* Ung., *Odontopteris crenulata* Brgt., *Cordaites borassifolia* Ung., *Hymenophyllites furcatus* Brgt.: and *Sphenopteris latifolia* Brgt., all found there in connection with it, ascend to coal No. 1B or higher. Among the trees seen in this coal, and ranging still higher in the measures, there are six species of *Lepidodendron*, two of *Sigillaria*, two *Calamites*, *Stigmaria* and its varieties, and a few species of *Carpolithes* and of *Cardiocarpon*. Though this subconglomerate coal has not been yet explored over a large area, it shows already more than twenty species, representing all the essential genera of our coal-plants, which have a wider range of distribution, and appear still in the coal strata above the conglomerate. It is therefore evident that a separation of the subconglomerate coal, as a peculiar formation and under a peculiar name, is contrary to palæontological evidence. Like every other coal bed the subconglomerate coal has species peculiar to it. Two species of *Lepidodendron*, two of *Sigillaria*, one of *Sphenophyllum*, two species of *Trigonocarpum*, one very large *Cardiocarpum*, one *Stigmaria*, one *Alethopteris*, and three or four *Sphenopterideæ*.* In the shales of this lowest bed of coal, near Frog

* All the new species of this coal, or at least most of them, belong to the State Geological survey of Arkansas, and are reserved for publication in the second volume of that Report.

Bayou, Arkansas, I have found for the first time in America one of those beautiful wings of insects, referred by M. Germar to the genus *Blattina.* Though related to *Blattina didyma* Germ., our American species is new.

The first bed of coal above the conglomerate, our No. 1A, is generally thin (two feet to four feet thick at the most), and overlaid by a stratum of coarse sandstone or of black very bituminous shales. In the shales, I have never found any other remains of plants, than leaves and cones of *Lepidodendron*, six species of *Lepidostrobus* and *Lepidophyllum.* The shales are too bituminous for good preservation of specimens. With the remains of plants, the shales generally contain a few specimens of *Lingula umbonata* Cox, a shell extremely abundant in the shales of No. 1B, and rarely represented by a very few individuals in the coal No. 2. Sometimes coal No. 1A and No. 1B become united together, being separated only by a thin parting of shales, but more generally there is between them a stratum of coarse sandstone, marked with numerous prints or casts of great vegetables of the coal, especially of *Lepidodendron obovatum* Sternb., *L. rimosum* Sternb., *L. rugosum* Sternb., *L. aculeatum* Sternb., *Sigillaria alternans* Ll. and Hutt., *S. reniformis* Brgt., *S. Brardii* Brgt., *S. lævigata* Brgt., *Sirigodendron pachyderma* Brgt., and some fruits, *Carpolithes* and *Trigonocarpon.* Other species, one *Caulopteris*, one *Megaphytum*, some *Calamites*, all rendered undeterminable by the coarse compound of the sandstone, have been seen in this stratum.

Coal 1B. Its horizon appears to mark the epoch of the highest development of the vegetation of the coal formation. Not only is this coal bed the most reliable of all, and consequently the most extensively worked, not only does it attain locally an enormous thickness, justly meriting the name which it bears in some parts of Pennsylvania, viz., the *Big Coal*, the *Mammoth vein*, &c.; but the shales which cover it and sometimes divide it into two, three, or more members, afford to the explorer the greatest amount of species, distributed among nearly all the genera of plants which belong to the coal formation.

The vegetation of coal No. 1B may be characterized: First, by the great number of fruits, found in the strata connected with it, either in the shale above or in the sandstone below. Indeed, nearly all the species of *Trigoncarpum*, *Cardiocarpum*, *Rhabdocarpos* and *Carpolithes* belong to it. Its second characteristic, is the great abundance of species of *Lepidodendron.* Eighteen species of this genus have been found in this bed of coal, and no new species have been, as yet, seen above it. A third character is the constant presence in the shales of our No. 1B of *Alethopteris Lonchitidis* Brgt. which apparently belongs exclusively to it. Generally speaking, the coal has the great forms of the section of the *Pecopterideæ*, viz. *Alethopteris* and *Callipteris*, and is mostly

deprived of the true *Pecopteris* or of the small forms of the order. *Alethopteris Serlii* Brgt., is met in its shales, but is very scarce; the species rather belongs to No. 3. It has *Alethopteris marginata* Brgt., not seen in any other horizon; also *Alethopteris lævis* Lsqx., probably peculiar to it, and *Alethopteris nervosa* Brgt., which ascends higher. Besides the genus *Pecopteris* Brgt., I cannot mention any species peculiar to this coal. *Pecopteris velutina* Lsqx., a fine species, which by its fructification would be referable to a separate genus, was found in its shales; but we have mentioned it also with the subconglomerate coal. *Pecopteris Miltoni* Brgt., or *P. polymorpha* Brgt. (probably the same) is a species found sometimes in its shales; but it is common in the whole thickness of the coal measures. *Pecopteris pennæformis* Brgt., *P. Sillimanni* Brgt., *P. plumosa* Brgt., *P. villosa* Brgt., are connected with it, but are found still higher in the measures. The fourth character of this coal is the great number of species of large *Sphenopterideæ* and of *Hymenophyllites* connected with it. *Sphenopteris latifolia* Brgt., *S. obtusiloba* Brgt., *S. glandulosa* Lsqx., *S. polyphylla* Ll. and Hutt., *S. Newberryi* Lsqx., *S. artemisiæfolia* Brgt., with *Hymenophyllites Hildrethi* Lsqx., and *H. spinosus* Göpp., have not been hitherto found except with this coal.

Of the genus *Odontopteris* Brgt., all the American species are connected with No. 1B, except *O. crenulata* Brgt., which first appears in the subconglomerate coal.

Among the *Sigillariæ*, it has as species peculiar to it, *Sigillaria stellata* Lsqx., *S. Serlii* Brgt., *S. tessellata* Brgt., *S. Brochanti* Brgt., *S. alveolaris* Brgt., *S. oculata* Brgt., *S. polita* Lsqx., *S. obovata* Lsqx., *S. alternans* Ll. and Hutt., *S. discoidea* Lsqx., and *S. catenulata* Ll. and Hutt.

By far the greatest number of specimens preserved in the shales of this coal belong to two species which we have seen already below the conglomerate, viz. the omnipresent *Neuropteris hirsuta* Lsqx., and *N. flexuosa* Brgt. Although these two species are so abundantly represented in our coal No. 1B, that sometimes their remains exclusively, fill the roof of some mines, other species of *Neuropteris* are scarcely found in connection with it. *Neuropteris tenuifolia* Brgt., and *N. smilacifolia* Sternb., belong to it; but both species are very scarce and closely related to *N. flexuosa* Brgt. The last especially may be considered as a variety of it. *Neuropteris lancifolia* Newb. is an undescribed species, found in the shales of this coal, perhaps only one of the numerous forms of *N. hirsuta* Lsqx.

Connected with this remarkable coal bed, we still find *Cordaites borassifolia* Ung., and *Dictyopteris obliqua* Bunby., locally distributed; *Whittleseya elegans* Newb., formerly described and found at one or two places only; species of *Asterophyllites*, *Annularia* and *Sphenophyllum*, but none of them peculiar to it, and species of *Calamites*, especially *C. Suckowii* Brgt., *C. Cistii*

Brgt., *C. nodosus* Brgt., *C. approximatus* Brgt., and *C. cannæformis* Brgt., some of which ascend higher in the coal measures. I never found in it any specimen referable to *Calamites pachyderma* Brgt., which M. Geinitz unites, I think incorrectly, to *C. cannœformis* Brgt. Its true place is within the sandstone of the Millstone grit. Thus also, from dissimilarity of distribution, I would admit with Mr. Brongniart *Calamites decoratus* Brgt., and *C. Suckowii* Brgt., as distinct species, though they are also united by M. Geinitz; the last belonging especially to this coal, the former to a higher bed, No. 3d.

It is perhaps useless to mention that *Stigmaria ficoides* Sternb., is also present with our coal 1B. It has not only penetrated by its leaves the fire clay which the coal overlies; but has filled the coal also by its remains:—moreover, contrary to the assertions of some authors that *Stigmaria*, being a root, is found only in the bottom fire clay of the coal strata,—it has mixed its remains in the shales of this bed, and even sent them into the sandstone by which they are overlaid, or sometimes replaced. Thus, in the absence of other remains, or until I had discovered them, the abundance of *Stigmaria ficoides* in the roof-shales has helped me to identify this coal in many places.

The roof shales of No. 1B coal are occasionally overlaid by a stratum of limestone or argillaceous shale, containing a great abundance of fossil shells. Locally also, and where this limestone is absent, its place is taken by a bed of coal, variable in thickness from one to four feet. As it is rarely formed, and sometimes in close proximity to No. 1B, I consider it still as a member of No. 1B; and it is accordingly ranked in my sections as No. 1C. Though it has no great value,* its combustible matter being a compound of half decomposed stems, mostly transformed into charcoal and rusted by oxyd of iron, it is worth mentioning as an evidence of conformity of composition of the shales and of the coal strata at the same geological horizon over a wide area. The shales of this bed, even the coal itself, appear like a compound of broken, heaped up stems of ferns and *Calamites*. Now, it is found with exactly the same appearance, either of shales or of coal, on the western limits of the eastern coal basin of Kentucky, near Greenupsburg; in Virginia, on the Tug river; and in the middle of the great Apalachian coal basin, near the mouth of Yellow creek, in the Ohio river, above Steubenville, Ohio.

Coal No. 2, is generally placed at about one hundred feet above No. 1B, and separated from it by various strata of shales and sandstone, and occasionally by the limestone mentioned before, or also by a cherty compound named Burrstone. Its roof-shales

* It is particularly well developed in the northern part of the coal basin of Illinois and Indiana where I have seen it lately from four to six feet thick of pretty good coal.

are coarse, micaceous, and generally barren of fossil remains; but it is separated into two by a shale parting, of from two to eighteen inches, which contains a great abundance of broken remains of plants. Here, we have scarcely any trace of *Lepidodendron*, but a remarkable abundance of *Calamites*, *Asterophyllites*, *Neuropteris flexuosa* Brgt., *Neuropteris hirsuta* Lsqx., *Cordaites borassifolia* Ung. I have found with this coal *Lepidodendron obovatum* Sternb., *Sigillaria Brardii* Brgt., with its leaves, and an undescribable *Lepidostrobus* Brgt. Without doubt, it has many peculiar species, but I have never found the shales hard enough for preserving specimens even of a moderate size. As soon as they are exposed to atmospheric influence, they are rapidly broken into minute particles by efflorescence of the sulphuret of iron, generally predominant in this coal. Its vegetation, as far as can be seen, is intermediate between No. 1B and No. 3d coal, the number of predominating species being from the genera *Asterophyllites*, *Annularia*, and especially *Calamites*.

Coal No. 3 is variable in thickness, and is not as generally and as uniformly formed as No. 1B. Wherever it has been seen it is overlaid by a roof of gray soft shales containing a great quantity of well preserved remains of plants. It has still a few species of *Lepidodendron: Lepidodendron dichotomum* Sternb., and especially its branches, are abundantly distributed in its shales, with a great number of its long cones, *Lepidostrobus variabilis* Brgt., and its leaves. *Dictyopteris obliqua* Bunb. is still found connected with it and locally distributed as in coal No. 1B. It has also in abundance *Neuropteris hirsuta* Lsqx., and *N. flexuosa* Brgt., and as peculiar species of this genus, apparently belonging exclusively to it, *Neuropteris Clarksoni* Lsqx., *N. gigantea* Sternb., *N. delicatula* Lsqx., and *N. rarinervis* Bunby. The genus *Sphenopteris* Brgt. is represented in this coal by *Sphenopteris flagellaris* Lsqx., *S. Lesquereuxii* Newb., and by an *Hymenophyllites*, intermediate between *H. elegans* Brgt., and *H. Hildrethi* Lsqx., and which is perhaps only a variety of the last species. The section of the large *Pecopterideæ* shows in it numerous remains of some of its species, especially *Alethopteris Serlii* Brgt., *A. nervosa* Brgt. and *A. muricata* Göpp. This last is uncommon and has been found only in connection with this coal. The species of true *Pecopteris* are more numerous than in coal 1B, beginning here to show their predominance. *Pecopteris unita* Brgt., *P. polymorpha* Brgt., *P. Loschii* Brgt., *P. Sillimani* Brgt. are connected with this bed, though none of them except perhaps the first is peculiar to it. *Asterophyllites*, *Annularia* and *Sphenophyllum* have left their remains in its shales, all still more abundant in No. 4 coal, except *Annularia sphenophylloides* Ung., which attains here its greatest development. Among the *Calamites*, it has *Calamites decoratus* Brgt., *C. undulatus* Brgt., the first apparently peculiar to it, with *C. cruciatus* Brgt. and *C. ramosus* in abundance. Thus

far, I have never found with this coal *C. Suckowii* Brgt., which has left so many of its remains in coal 1B. A few of the most common species of *Sigillaria*, viz. *S. Brardii* Brgt., *S. Menardi* Brgt., *S. reniformis* Brgt. and *S. Yardleyi* Lsqx., all species (except *S. Menardi* Brgt.,) which ascend higher or are found also in No. 1B. To this enumeration, I may add two species of fruits, *Carpolithes amygdaliformis* Göpp, and a small *Cardiocarpon*, fruit of an *Asterophyllites*. It is found also with No. 4th coal.

Coal No. 4th is the coal of the small ferns, especially of the small *Pecopterideæ*. It has no more remains of *Lepidodendron*, neither cones, nor leaves of this genus. Still it has some *Sigillariæ*. *Sigillaria lepidodendrifolia* Brgt., *S. Brardii* Brgt., *S. Defrancii* Brgt., *S. sculpta* Lsqx., *S. obliqua* Brgt.; •*S. dilatata* Lsqx., *S. fissa* Lsqx., *S. Schimperi* Lsqx. These five last species are peculiar to it. All belong to the sections of the uncostate *Sigillariæ*. Among the costate species, none have been found in the shales but *S. lævigata* Brgt. The remains of *Asterophyllites* and its cones, of *Sphenophyllum* and especially of *Pinnularia capillacea* Ll. and H., are extremely numerous in its shales. This last species scatters its threadlike branches in every direction and mixed with *Neuropteris hirsuta* Lsqx.; and *N. flexuosa* Brgt. still common species of this coal, covers the roof of the Pomeroy vein with a painting of beautiful garlands. *Dictyopteris obliqua* Bunby., still appears with this coal, but is very scarce indeed, a species apparently dying out. Now the *Neuropterideæ* become more predominant and varied than in any other horizon. Thus, *Neuropteris speciosa* Lsqx., the most beautiful of the genus, *N. fissa* Lsqx., *N. plicata* Sternb., *N. rotundifolia* Brgt., *N. Grangeri* Brgt., *N. Cistii* Brgt., *N. Villersii* Brgt., *N. gibbosa* Lsqx., *N. undans* Lsqx., *N. tenuinervis* Lsqx., *N. dentata* Lsqx., *N. Desorii* Lsqx., *N. heterophylla* Brgt., all these species, except perhaps the last, appear for the first time, and are probably contemporaneous with this coal only. It has still *Neuropteris crenulata* Brgt., a species connected also with No. 1B. The *Sphenopterideæ* are represented here by *Sphenopteris tenella?* Brgt. (The only specimen found was indistinct.) *S. Gravenhorstii* Brgt., *S. Dubuissonis* Brgt., *S. abbreviata* Lsqx., *S. intermedia* Lsqx., *S. plicata* Lsqx., all species of the same type with small dissected leaflets, far different from the characteristic type (the large leaflets) of the species of No. 1B. To this coal belong exclusively all the species of doubtful affinity, referred by some authors to *Aphlebia* Sternb., and connected in my catalogue with *Hymenophyllites* Göpp., viz. *Hymenophyllites fimbriatus* Lsqx., *H. affinis* Lsqx., *H. hirsutus* Lsqx. and *H. giganteus* Lsqx.

The *Pecopterideæ* have the first rank for the number of representatives in coal No. 4th. It has *Alethopteris Pennsylvanica* Lsqx., *A. aquilina* Brgt., *A. urophylla?* Brgt., (specimens incomplete,)

A. obscura Lsqx., *A. serrula* Lsqx., *A. rugosa* Lsqx., with *Pecopteris arborescens* Brgt. (abounding), *P. notata* Lsqx., *P. distans* Lsqx., *P. oreopteridius* Brgt., *P. pusilla* Lsqx., *P. dubia* Lsqx., (referable with doubt to *P. arborescens* Brgt.,) *P. cyathœa* Brgt., *P. arguta* Brgt., *P. concinna* Lsqx., *P. incompleta* Lsqx., mostly species peculiar to this coal. *Pecopteris polymorpha* Brgt. is also found in its shales.

The genus *Calamites* is here represented by *Calamites cruciatus* Brgt, *C. dubius* Brgt., *C. bistriatus* Lsqx., *C. disjunctus* Lsqx. These two last species have been found only with this coal; but each has been established on single specimens and thus they are still uncertain.

From this enumeration, it is evident that we have passed at this geological horizon to a vegetation much reduced in the size of its representatives and of a quite different character. The arborescent species belong to the ferns, or to *Sigillaria*, of small size.

Our 4th coal is covered, as was said before, by a thick stratum of sandstone, generally conglomerate in part, varying in thickness from 10 to 150 feet. The vegetable fossil remains of this sandstone afford a new evidence of the predominance of the ferns at this geological horizon, and of the disappearance of *Lepidodendron*. It is the immediate deposit of silicified trunks of *Psaronius*, extending from Ohio to Virginia along the Great Kanawha river. At Shade river, near Athens, Ohio, the broken pieces of these trees are so numerous that they cover in places the bed of the creek.* They appear generally as pieces of small stems horizontally broken, varying from two to eleven inches in thickness. The largest specimen which I was able to find is apparently the base of a tree. It is nearly round, eleven inches in diameter, and perforated, not in its central part, but somewhat on one side, by a regular round hole four inches in diameter. Many of the small specimens have their surface regularly costate like species of *Sigillaria;* but their internal structure is apparently that of *Psaronius*. It would be very interesting to compare with ours the position of the *Psaronius* deposit of Europe, which, I suppose, is contemporaneous.

The five hundred feet of measures intervening between the Mahoning sandstone and the great Pittsburg coal, have not afforded hitherto very abundant materials for the study of their botanical remains. It has been seen already that, in Pennsylvania, this space is occupied by strata of shales, sandstone and limestone which are generally barren of coal, at least of

* Unhappily, I have not as yet been able to obtain for microscopical examination polished lamellæ of sections of the collected species and cannot give an exact account of them. But the work of preparing the specimens is now in progress, and the view of internal structure of those interesting remains will soon afford the possibility of determining the species.

any bed of coal of sufficient thickness to encourage the working. Thus the shales are still unopened and no chance is given of examining them. In Kentucky where in the same space the coal is more developed and has been opened at different places and different geological horizons, the shales roofing the coal-strata are mostly very bituminous and contain especially animal remains, fishes and shells, either with or without few fossil plants. Nevertheless I have been able to find here and there some determinable species affording points of comparison for the distribution of the coal-plants in those higher measures. Thus, the fire-clay of the bottoms of all the coal-strata is always filled with remains (leaves only) of *Stigmaria ficoides* Brgt., and at any place where the coal is formed, it shows on its horizontal sections the carbonized prints of *Calamites*, *Pecopteris* and *Stigmaria.*

Coal No. 6 is covered by a schistose, gray, laminated sandstone, blackened by broken fragments of ferns. Leaflets of *Pecopteris*, *Sphenophyllum*, *Asterophyllites* and crushed branches of *Calamites*, are distinguishable in great abundance among the fragments; but their specific determination is not possible.

Coal No. 8, has in its shales *Pecopteris polymorpha* Brgt. in abundance. Near Marietta, Ohio, its place is occupied by a red soft shale full of the remains of *Asplenites rubra* Lsqx., closely related to *Pecopteris arborescens.* *Neuropteris hirsuta* is there also in its normal form.

Coal No. 9. Its roof shales contain mixed together a great quantity of well-preserved fossil shells, of scales and teeth of fishes, and of remains of ferns evidently floated. Two species of *Sphenopteris*, one of which appears identical with *S. gravenhorstii* Brgt., *Pecopteris polymorpha* Brgt., another *Pecopteris* referable by the small fragments found, to *P. arborescens* Brgt.; *Neuropteris flexuosa* Brgt., leaflets only separated from the stem, a few undeterminable species of *Calamites* and one *Sigillaria.*

Coal No. 11. The great Pittsburg coal has also both animal and vegetable remains, but generally on isolated strata among the shales. At Pittsburg the lower bed is roofed by black bituminous shales which appear formed wholly by remains of stems of *Neuropteris hirsuta* Lsqx. and by leaves of *Cordaites borassifolia*, and in the shales of the upper part of this coal, which corresponds to our No. 12 and which in Kentucky is sometimes separated from No. 11 by a thin and very irregular bed of limestone, there are at Pittsburg well preserved specimens also of *Neuropteris hirsuta* Brgt., *N. flexuosa* Brgt., *Pecopteris polymorpha* Brgt., *P. arborescens* Brgt., *P. cyathœa* Brgt., *Sphenophyllum emarginatum* Brgt., three species of *Calamites* and one *Sigillaria.* The same species with a far greater proportion of *Calamites* and also one

*Sigillaria** are found at Paradise, on Green river, Kentucky, in the shaly or 'brashy' coal separating No. 12th coal from an iron blackband which overlies it. I must not omit to mention with this coal the first appearance of a species somewhat related by its form to the species of the Oolitic series. It is the remarkable *Neuropteris Moorii* Lsqx., which by its pointed leaves and alate primary or secondary rachis, is distinct from any typical form of the coal-plants. It may be compared by its general appearance to *Alethopteris Whitbiensis* Brgt. of the Oolite of England but the affinity is distant indeed, as the American species has no marked medial nerve and some of the forked and arched nervules of its leaflets emerge from the common rachis.

The third great sandstone formation, or the Anvil-rock sandstone, which underlies the highest section of the coal-measures, is from all appearance as distinct a point of division in the distribution of the fossil flora as any one of the other great sandstones, the conglomerate and the Mahoning. From my own observations and from the data collected by other geologists, especially by my friend J. P. Lesley, no trace of *Lepidodendron* appears either in the Anvil-rock sandstone or in the measures above it. At least, none has been found as yet. This sandstone nevertheless is not barren of fossil vegetables. Near Greensburg, Penn., it contains in abundance, petrified trunks half silicified, half transformed into sandstone. Among the specimens which I had an opportunity to examine in the cabinet of Prof. Moore, I found a portion of the base of a small tree which had been evidently imbedded in the sandstone, in its standing position, still preserving the embranchment of three diverging roots. The surface of the tree and of the roots is marked by peculiar scars. Though not perfectly distinct, it was at once evident that this fossil tree did not belong to any of the genera of plants before seen in the coal. It was nevertheless impossible to describe it, in its state of obliteration. Later, Dr. D. Dale Owen, the celebrated geologist of New Harmony, discovered in the Anvil-rock sandstone of Posey Co., Indiana, not far from the Wabash river, three or four standing stumps which by examination proved to belong to the same species of trees as the one found at Greensburg. The specimens of Dr. Owen have been carefully removed and are preserved in his cabinet, with the roots attached to them, just as they were found. The stem is round, about one foot thick, branching at its base in five or six large roots, diverging nearly horizontally. Its surface is narrowly reticulated, and is marked by double oval scars, united by a deep wrinkle, very regular in their position, and about one inch distant from

* It is a costate species which appears to be the same at No. 9th and No. 12th coal. All the specimens have lost the bark and I could never find the scars except those which were too much obliterated for satisfactory determination.

each other. At the point where the roots branch and diverge from the trunk, the double scars become more and more separate, more irregularly placed, and thus the roots take somewhat of the appearance of *Stigmaria*. The difference is well marked, nevertheless; the scars being triangular, marked in the center by a deep point only, and the roots quickly diminishing in size and terminating in a point at a short distance from the base of the tree. The imbedding shales not having been preserved, there is no trace of rootlets. Thus we have here, at this high position in the coal measures, a new typical form which probably indicates a difference of vegetation in the subsequent and last stage of the coal formations.

This enumeration is already too long, and though still incomplete, it must be abruptly closed, for fear of becoming tedious to the reader. In order to be understood by those who are not acquainted with botanical palæontology, it is only necessary to sum up and briefly discuss some of the conclusions which are derived from this examination.

Considering its generic distribution, the American coal-flora is nearly related to the European. We have only two or three peculiar genera, representing distinct types, which have not been seen in Europe. On the contrary, Europe has no peculiar and true generic types of coal-plants which are not represented in the coal-fields of the United States.

Considering its species, a more marked difference in the coal-flora of both continents becomes evident. Some of our species represent marked and peculiar forms or types, which are not seen in Europe, though a much greater number of species has been found in its coal-measures. Thus the predominance of typical or distinctly characterized forms, belongs to our country. By comparison of the flora of our epoch on both continents, we find now the same proportional relation and difference as at the time of the coal formation, that is, on this side of the Atlantic a predominance of well marked types; a predominance of species of trees;* a number of species perfectly identical on both continents, and many American species so nearly related to European congeners that their specific characters can hardly be established.

Though further researches ought necessarily to increase the number of species of fossil plants belonging to our coal measures, the proportional difference is likely to remain as it is established above.

The fossil-flora appears identical at the same geological horizon, over the whole extent of our coal-fields. This proves uniformity of stratification and geological unity of the different coal basins of America.

* The distribution of the genus *Lepidodendron*, at the time of the formation of the coal, has some analogy with that of the Oak in our time.

The first trace of vegetable terrestrial life appears in the middle of the Devonian in a species of *Lepidodendron*, represented by its bark, its leaves, its cones, and large trunks of silicified wood. No remains of any other form of terrestrial vegetation have been seen in strata either inferior or cotemporaneous to this. All the vegetable remains known in the Silurian and lower Devonian belong to species of fucoides or marine plants, mostly of small size, resembling some species of *Fucus* of our time. The first leafy terrestrial plants appear in the Old Red sandstone. All the representatives of this new vegetation belong to a peculiar genus, *Noggerathia* Göpp., more related to Conifers or even to Palms than to Ferns. They are found in the same geological horizon, both in Europe and in America, and entirely disappear at or before the beginning of the coal epoch.

Ascending to the base of the coal-measures, we find there simultaneously in the subcarboniferous sandstone and below the upper Archimedes limestone different species of trees of large size. At the base of the Millstone-grit, where the coal begins to be formed, the number of species of large sized trees, especially of *Lepidodendron*, increases. At the same time, we see here the first species of ferns belonging to the true coal-measures, and already some species which reappear periodically with each bed of coal, in the whole thickness of the coal-measures.

At coal No. 1B, the second above the Millstone-grit, we have apparently the maximum of predominance of species of large size, especially of *Lepidodendron*. From this horizon upwards, *Megaphytum*, *Ulodendron*, *Halonia*, *Caulopteris*, *Lomatophloios* and *Knorria* are not seen any more. The species of *Lepidodendron* diminish in number to coal No. 4, where they are entirely lost; at least they have not been found as yet in any strata of the coal-measures above it. The genus *Sigillaria* follows also from coal 1B the same gradual diminution in the number of its species; but it is preserved in one or two representatives as high as coal No. 12.

As fast as these species of trees decrease in number, the ferns mostly of small size invade the coal-fields. They become predominant and show the greatest number of species at the base of the Mahoning sandstone.

This substitution of species is not the result of any perceptible change in their character or in their relative affinity. The relation of *Lepidodendron* is to *Lycopodites*. Both genera appear or at least disappear at the same time, and are replaced by typical forms, which have no analogy whatever with them.

The distribution of the ferns in the coal-measures is equally contrary to the supposition of a change of species by successive variations. They appear, it is true, grouped together, in a kind of relation between contemporaneous species; but we do not see, either before or after any of them, a trace of an intermediate

form between the lost types and the following ones. The large-leafed *Sphenopterideæ*, the *Odontopterideæ* belong to coal No. 1B with most of the fruits of the coal measures; the *Neuropterideæ*, the *Pecopterideæ* and a peculiar section of small leafed *Sphenopterideæ* belong to coals No. 3 and 4.

As if to show how useless it would be to argue on the distribution of the coal-flora as resulting from successive variations of species and of genera, we find predominant genera represented in the whole thickness and in the whole extent of the coal-fields by species so variable that they can be called polymorphous, and which nevertheless preserve everywhere their identity. Thus appear *Neuropteris hirsuta* Lsqx., *N. flexuosa* Brgt., *Pecopteris polymorpha* Brgt. In the palæontological report of the Pennsylvania geological survey, I have figured eighteen forms of the first of these species, passing by insensible transitions from a small round leaflet to a large, nearly square *Cyclopteris;* then to cordate or reniform leaf of medium size; then to opposite, oval-lanceolate leaflets united by a narrow margin; then to a digitate leaf of which the five divisions are lanceolate-obtuse, and thus *ad infinitum.* Nevertheless, this species is perfectly well characterized and may be identified at first sight in any of its multiple forms.*

There is not in the number of *Neuropterideæ* and *Pecopterideæ* a well characterized species which could be admitted as a modified form of the predominant and variable species. Moreover, the numerous species of *Neuropteris* and *Pecopteris* appear at coal No. 3 and 4 in the middle of the coal-measures, and do not ascend higher, while those species which should be considered as originators or parents and consequently ought to be destroyed (from the law of selection) by their offspring, continue to predominate to the top of the coal-measures. Thus the vegetation of the coal shows at different geological horizons, both a gradual and sudden disappearance of species and even of types; both a gradual and sudden appearance of other species or of peculiar types, without regard to the former or extinct ones, and a continual reappearance in the whole thickness of the coal-measures of well-established species which neither by their form or their nature appear particularly prepared to sustain without varying the successive changes which have acted on the surface of the coal-measures at the time of their formation.

It is certainly possible to suppose that some disturbance, immersion, or upheaval of the coal-marshes, has modified their

* As the specimens are mostly found in simple leaflets detached from the stem, and consequently the species cannot be identified in its varieties by the examination of a stem bearing leaves of different forms, this facility of identifying any single leaflet may answer the reproach made to palæontological Botany, that it is a science directing its researches to variable parts of beings of which the true nature and real form is generally unknown, and that consequently its conclusions are more or less problematic. This reproach was long since conclusively answered in a letter of Prof. Heer to Sir Charles Lyell.

vegetation. But this supposition would not explain how it happened that the most obvious changes in the vegetation of the coal have not followed the formation of those great strata of sandstone, which, like the Millstone-grit or the Mahoning sandstone, must have employed a longer time than any other stratum for the gathering up of their materials.

The supposition may be further extended, and it may be asserted that the disappearance of certain types, or of vegetables of a large size, from the marshes of the coal-fields is only apparent. That in their forced migrations by the gradual submersion of the marshes, some species have been either arrested somewhere, and thus left inhabiting other countries or destroyed for a time by a too slow migrating process, while others, having the faculty of migrating developed in a larger degree, have reappeared again and again on the marshes, living there for a longer period.

The field of supposition is unbounded, but it is a field where science is not permitted to wander. Where have the new types or species come from? How is it, if the disappearance of vegetables like *Lepidodendron* is only apparent, or local, that we do not find any of their remains succeeding the carboniferous epoch.* And admitting that species of *Lepidodendron* have been arrested in their migrations on some dry land, should we not find above our 4th coal some remains of this genus in the strata of sandstone where so many trunks of *Psaronius* have been imbedded.

I wish that I had time to discuss here at length and with the attention which it merits, a subject of importance connected with the examition of the stratigraphical distribution of the plants of the coal-epoch. Are the coal-measures a single, unique formation? Do they belong to a single epoch, or are they composed of a succession of formations separated by immense space of time, and of which the different stages might be compared to those of the recent formations: the Eocene, the Miocene, and the Pliocene, for example? In the last case, can we admit the vegetation of which the remains have been preserved in the shales of the coal, or the vegetation of the coal-marshes, as a true representative of the flora of the various epochs where the coal was formed; or was it then, as the bog vegetation is at our time, composed of a peculiar group of plants, adapted to the formation of the coal, pertaining to the marshes only, while another

* A single specimen of *Stigmaria* is said to have been found in Germany in the Todtliegende or Permian. But the locality has not been ascertained and consequently the statement cannot be relied upon. Moreover for a long time and in many places, as I have seen it myself, the Permian in Germany was taken for the Old Red sandstone and vice-versa, from the difficulty of ascertaining its position and from the want of fossil remains. I cannot take into account as contradicting my assertion, one *Lepidodendron* mentioned by Mr. Murchison as found in the Permian of Russia.

flora of a different character was covering the dry land, if there was any, dry land, at the carboniferous epoch?

From the thickness of some beds of coal, representing a mass of combustible matter as great at least as that which is contained in our oldest and deepest peat bogs; from the thickness and various composition of the strata which separate the beds of coal, and from the successive changes in the vegetation of the coal, it appears that the last alternative is admissible. Different hypotheses have been put forward to explain the so-called huge or gigantic vegetation of the coal-formations. But there is nothing in the carboniferous epoch authorizing the supposition that the power of vegetable life was greater than it is at our time. The combustible matter heaped in some of our peat bogs is sometimes sufficiently thick to be equivalent to the coal of a bed of four to five feet. The trees growing in our marshes or on the peat bogs are generally larger than those which have been preserved in the strata of the carboniferous measures. The Dismal Swamp is impenetrable on account of the great compactness of its vegetation. It is not an easy matter either, to get across the heaped, half prostrated or erect and closely pressed trees of our cedar-swamps of the North. If such marshes were extended over the greatest part of the United States, they would present a fair representation of those of the carboniferous period.

The occasional appearance of petrified trees, standing imbedded in sandstone, does not give evidence of a rapid formation either of the coal or of the other strata. Local disturbances may throw a few feet of sand upon a marsh, covered with active vegetation, and thus preserve the stumps from decomposition and by-and-by these may be converted to stone. The bald cypress and other species of trees grow sometimes in the marshes near the sea shore under ten feet of water. Whole forests of those trees have been imbedded in a standing position in the marshes around New Orleans. Thus I do not find in the geological records of the carboniferous period any indication of a rapid process of formation, either cataclysmic or abnormal, and I readily admit that each bed of coal with its accompanying strata of fire-clay and shales has required for its formation a period of time as long as any of our recent geological divisions.*

The question concerning the existence or non-existence of dry land covered with a peculiar vegetation at the epoch of the coal formation, cannot be answered positively or negatively by sufficient evidence. The only fact that would indicate that the

* Thus, if a peculiar nomenclature for a classification of the different strata of the old red sandstone, of the subconglomerate coal, and of the Millstoue-grit is admissible, the process of division should be extended to each bed of the coal-measures.

marshes of the carboniferous epoch were surrounded by land-bearing plants of different kind than those living on the bogs is the presence in coal No. 1B and in the sandstone underlying it of a great number of fruits of different species which by their nature have no relation to any of the other remains preserved in the coal. They have been generally referred to species of *Cordaites.* But the two only species of our coal measures are found in abundance at geological horizons where the fruits are entirely absent. And even at coal No. 1B shales appearing entirely composed of heaped remains of leaves of *Cordaites borassifolia* do not contain any fruit. The species of fruit, *Carpolithes Cordai* Gein., referred by M. Geinitz to *Cordaites borassifolia, our most common and omnipresent species*, has not been found in the coal measures of America. Therefore, either the fruits of unknown relation, *Carpolithes*, *Trigonocarpa* and *Rhabdocarpos** belong to vegetable species which have grown on the marshes, and of which the remains, leaves and stems, have been entirely obliterated or those fruits belong to species growing out of the marshes, around them, and have been floated and thus disseminated in the shales and in the sandstones. This last opinion appears at first confirmed by a similar process of distribution of species in our deep swamps. I have already mentioned elsewhere† how the hollow trunks of the bald cypress which grows in Drummond lake (Dismal Swamp of Virginia) are filled by fruits, acorns, nuts, &c., of trees which grow on the dry land near its borders. But, it is not presumable that species of fruits only could have been floated and disseminated by the agency of water without any of the branches and of the leaves of the plants to which they belong. And nowhere have the shales, covering what is called the tail of a coal bank, viz: the part abutting against a hill of sand or losing itself in sandstone, exposed any remains of plants of another type than those belonging to the true coal formation. Even where the shales of the coal are covered with remains of shells and of fishes, and consequently formed when the marshes were immersed, all the floated remains of plants which are found with those of animals belong to the common species of the coal. I believe then that the plants preserved in the shales of the coal give us a fair representation of the general flora of the carboniferous epoch, as true and as general at least as the fossil plants of the miocene represent the general flora of the tertiary period. And I suppose that if there was any dry land around the marshes, the vegetation contained only a few species different from those living on the marshes. But this last opinion is merely hypothetical.

[*To be continued.*]

* I consider the *Cardiocarpa* as the fruits of Asterophyllites and probably of some species of Calamites.

† Introduction to the fossil flora of Pennsylvania, Geol. Rep. of Penn., p. 847.

ART. XXXIV.—*Additional observations on the Circulation in the Eye;* by OGDEN N. ROOD, of Troy University, N. Y.

IN a notice which appeared in the September number of this Journal, I described a subjective phenomenon, which is seen when a bright sky is viewed through two or three plates of cobalt glass. Most persons who are in the habit of directing their attention to this class of phenomena, have even perceived *faint indications* of the circulatory movement with the naked eye. But in the experiment with the cobalt glass, where this motion is seen with distinctness, as well as in the faint indications of it obtained with the unassisted eye, the moving bodies are not observed to be in compact masses, but as it were, sprinkled over the field of view.

If however the light of a spirit-lamp with a salted wick, be concentrated on the eye by means of a convex lens, 3 inches in diameter, having a focal length of about 3 inches, the bright field soon resolves itself into a mass of small, round, *densely packed* moving bodies, which appear light on a dark ground.

This is seen with varying degrees of readiness by different persons: some perceiving it in a few seconds, others requiring a protracted gaze of several minutes.

The moving bodies at first appear very closely packed together like fine mosaic-work, but as the view grows more distinct, their path can be traced, and the conviction is forced on the mind of the observer, that they are moving at slow uniform rates, through narrow channels; the whole reminding one strongly of the circulation seen in the web of a frog's foot, by a microscope slightly out of focus.

The interposition of plates of yellow glass rather adds to the distinctness of this appearance.

The indistinct vision attendant on faintness or on mere eye-weariness, is, I suspect, in many cases, mediately caused by the appearance of this circulation: for example, some weeks ago, I tried the experiment of binding up the right eye, and using only the left, for two days. After reading with left eye for half an hour, the page grew indistinct, the letters apparently dissolved and this dense circulation, which I had previously observed, set in, and continued to be visible for some minutes. It reappeared a number of times during that, and the following day.

Subjoined is an extract of a letter from Prof. Wm. B. Rogers.

"Your experiment with the blue glass appears to render much more *distinctly visible* a phenomenon which I have often observed and which has been noticed by others. After a continued effort of vision in experiments on binocular combination or in the use

of the microscope as well as after active exercise, or any other stimulus to the circulation, I am able to reproduce it easily and with great clearness.

"When in these conditions I look intently with the naked eye upon a bright surface as that of a white cloud or a sheet of letter paper in the sunlight, the first appearance presented is that of numerous bright points darting around in various broken curves, coming in view and disappearing fitfully, but in such positions as to indicate the recurrence of the same motions, or the passage of successive particles in certain prescribed and permanent channels.

"As the eyes continue to be fixed on the surface, a shade comes over it, and on the dark ground I see innumerable streams of particles, moving in infinitely various loops and other curves, which, by a little attention, are observed to maintain a constant pattern, strikingly analogous to the capillary circulation as seen in the microscope. These streams of particles are of a tawny yellow tint well contrasted with the dark brownish surface in which they appear.

"Under the most favorable conditions of the organ I can usually obtain the effect with both eyes open, but it is more certain and far more distinct with a single eye, the other being closed. I should add that the appearance rarely lasts more than one or two seconds at a time, being obliterated by the recurrence of the white illumination and after a short interval returning, though usually with less distinctness than when first seen.

"In looking through a tube of black paper at a white surface until the eye has become fatigued, and we should perhaps add *greatly excited*, I have often noticed the same phenomenon and I believe I could at any time bring into view the yellowish mass of the circulation by continuing the experiment for a minute or two. With the lens of my pocket microscope held at about the focal distance I scarcely ever fail to obtain this effect in a few seconds.

"Your experiment with the blue glass is very interesting and succeeds with me perfectly. It will be a great gain if with comparatively little discomfort and without risk we shall be able to scrutinize steadily this intraocular phenomenon."

ART. XXXV.—*Some experiments and inferences in regard to Binocular Vision*; by Prof. WILIAM B. ROGERS.

IN the theory of binocular vision which has been so ably expounded by Sir David Brewster, Brücke, and others, it is contended that no part of an object is seen single and distinctly, but that to which the optic axes are for the moment directed, and that "the unity of the perception is obtained by the rapid survey which the eyes take of every part of the object." According to this view our perception of an object in its solidity and relief, instead of being the simple and direct result of the pictures formed at any one moment in the two eyes, is acquired by a cumulative process, in which the optic axes are successively converged upon every point of the object within view.

In an experimental discussion of some points in binocular vision published in the Am. Journ. of Science several years ago,* I was led to conclude that the phenomena of the stereoscopic resultant do not necessarily or even usually conform to these conditions, and that "the perception of a perspective resultant line or of a physical line in the same attitude does not require the successive convergence of the axes to *every point*." The grounds of this conclusion were,—first that the perspectiveness of the resultant although not perceived when the axes are steadily maintained at any one convergence, appears as soon as they are allowed sufficient freedom of motion *to unite a few contiguous points* of the component lines, and that it then effects the whole extent of the resultant; and—second that the resultant presents a perspective attitude even when the component lines, instead of being united into one, are brought together so as to intersect at a small angle, *each of the intersecting lines in this case appearing in relief*.

Satisfied from these considerations that the perceived singleness and relief of the resultant are to be ascribed rather to a *process of suggestion* then to the exhaustive binocular survey which has been supposed necessary, I was led to the opinion that, while some change of direction and convergence of the axes is needed for the effect, the successive view of a few contiguous points is all that is required to bring the suggestive action into play and to give rise to the full perception of the position and relief of the resultant.

In this view while rejecting the theory of successive vision in the form in which it has been propounded, I still considered some degree of motion of the axes as one of the steps by which we obtain the perception of the binocular resultant.

The following experiments, intended still further to test the theory of the successive combination of corresponding points

* This Journal, [2], xx, 86, 204, 318, and xxi, 80, 173, 439.

in binocular vision, are believed to be in part new, and are in part modified repetitions of experiments already described by Profs. Wheatstone and Dove. They offer what seems to be decisive proof that such a successive combination of pictures point by point, however it may enter in many cases into the complex process of vision, *cannot be regarded as an essential condition to the singleness and perspectiveness of the binocular perception.*

1. Let a brilliant line, held in a perspective position at a convenient distance midway between the eyes, be regarded intently for a few seconds so as to produce a lasting impression on the retinæ. On turning the eyes towards a blank wall or screen the subjective impression will be seen projected against it and having the same perspective altitude as the original line. If then one eye be closed the line will appear to subside into the surface of the screen taking an inclined position corresponding to the optical projection of the original line as seen by the unclosed eye and therefore corresponding to the image formed in that eye. By opening and closing the eyes alternately, and finally directing both to the screen we are able to see the two oblique lines corresponding to these projections and their binocular resultant corresponding to the original object.

2. Let two slightly inclined luminous lines formed by narrow slits in a strip of black card-board be combined into a perspective resultant either with or without a stereoscope. Looking at this for a few seconds so as to induce the reverse ocular spectrum, and then directing the eyes towards the opposite wall of the apartment, *a single spectrum will be observed having the attitude and relief of the original binocular resultant.*

As a strong illumination of the lines is necessary to bring out the most striking effect, the card-board should be held between the eyes and some brilliantly white surface, as the globe of a solar lamp or a strongly illuminated cloud, care being taken to prevent the entrance of extraneous light.

3. Using the same arrangement, let the luminous lines be regarded in succession each by the corresponding eye, the other eye being shaded so that no direct binocular combination can be formed. On looking toward the wall it will be seen that *the two subjective images unite to form a single spectral line having the same relief as if the lines had been directly combined* with or without the stereoscope.

While the perspective image continues distinctly visible, let either eye be closed the other being still directed towards the wall. The image will instantly lose its relief and take its position on the plane of the wall as an inclined line corresponding to the subjective image in the eye which has remained open. When the subjective impressions have been sufficiently strong, it is easy to alternate these effects by projecting first the picture

proper to the right eye and then that of the left on the plane of the wall, with their respective contrary inclinations. On then looking with both eyes we see the resultant image instantly start forth in its perspective altitude.

It is hardly necessary to say that in order to obtain these effects satisfactorily even with lines very strongly illuminated, the observer should have some practice in experiments on subjective vision. *In these circumstances, however, I have found the results to be perfectly certain and uniform.*

The conditions of the experiments are obviously such as to exclude all opportunity of *a shifting of the image on the retina.* Such a shifting however is essential to that successive combination of pairs of points in the two images which on the theory of Brewster is required for the production of the single perspective resultant. Hence according to this theory the resultant spectrum in these experiments, instead of being a single line in a perspective position, ought to present the form of two lines inclined or crossing, situated in the plane of the wall without projection or relief.

In reference to the first two experiments it might perhaps be maintained that as the perspectiveness of the original line or resultant on which the eyes were converged formed part of the direct perception in the first stage of the experiment, it would be likely through association to be included also in the spectral or subjective perception. But this consideration, which at best does not impress me as of much weight, is entirely inapplicable to the conditions of the last experiment, where the eyes are in the first place impressed *in succession* with their respective images, and where yet when they are together directed to the wall, *the perspective single resultant at once springs into view.*

4. Without resorting to these troublesome efforts of subjective vision the following is a simple proof that pictures *successively* impressed on the respective eyes are sufficient for the stereoscopic effect. Let a screen be made to vibrate or revolve somewhat rapidly between the eyes and the twin pictures of a stereoscope, so as alternately to expose and cover each, *completely excluding the simultaneous vision of the two.* The stereoscopic relief will be as apparent in these conditions as when the moving screen is withdrawn.

Here there is no opportunity for the combination of pairs of corresponding points in the two diagrams by the simultaneous convergence of the optic axes through them, but at each moment the actual picture in the one eye, and the retained impression in the other, form the elements of the perspective resultant which we perceive.

5. The ingenious experiments described by Prof. Dove many years ago in which the stereoscopic effect was obtained by the

momentary illumination of the electric flash, furnish a further and most powerful argument against the theory of successive binocular combination here referred to.

In repeating these I have found great advantage in using one of Ritchie's improved Ruhmkorff coils having a coated jar included in the outer circuit, the intensely brilliant spark of which can be made to throw its light upon the object viewed in any direction or at any interval that may be desired.

When a twin-diagram of any simple geometrical solid was placed in the stereoscope and viewed by this momentary light it was found to exhibit the perspective resultant in most cases with a single spark, and it never failed to present it in perfection with a succession of sparks even when they followed each other slowly.

A large circular disc of brass, marked with the usual concentric striæ, was placed in a position to catch the illumination and produce the peculiar intersecting lines of reflected light. At each spark the bright resultant line due to the binocular union of these intersecting lines was seen penetrating the disc and extending in a steep angle beyond and in front of it.

As, according to Wheatstone, the duration of an electric spark is less than one-millionth of a second, it can hardly be supposed that in either of these experiments the eyes have time to make the successive changes of direction required, by the theory, for the singleness and relief of the observed resultant. Not less at variance with this theory is the familiar fact that the illumination of a single flash of lightning is sufficient to give us a clear perception of the forms and positions of objects to which the eyes are for the moment directed. So the long straight spark of one of Geissler's narrow vacuum tubes, glowing for an instant in a dark room, impresses a precise perception of the altitude and place of the tube and its included line of light, and even their regular path of the long spark through the air produces a distinct perception single and faithful to its devious directions.

We may therefore conclude—*first*, that the perception of an object in its proper relief, or that of the perspective resultant through binocular combination in a stereoscope, or otherwise, may and most usually does arise, by *direct suggestion* from the two pictures impressed, without requiring the successive combination of corresponding points;—*and second,* that for the singleness of the resultant perception, it is not necessary that the images should fall on what are called corresponding parts of the two retinas.

The condition of single vision in such cases seems to be simply this, that the pictures in the two eyes shall be such and so placed as to be identical with the pictures which the real object would make at a given distance and in a given altitude before the eyes.

XXXVI.—*Geographical Notices.* No. XIV.

PROF. GUYOT'S MEASUREMENTS OF THE ALLEGHANY SYSTEM.—It is well known to the scientific men of this country that Professor Arnold Guyot of Princeton, New Jersey, has devoted a portion of his summer vacations for ten years past to the study of the different portions of the great Alleghany system which faces the Atlantic coast from Canada to Georgia. Several years ago he measured the highest peaks of the Adirondack, Green and White mountains, in the northern part of the chain, and more recently he has been at work on the southern portion of the system which is found to possess the most elevated peaks of the whole Appalachian chain. His determination of some of the highest peaks of the Black mountains of North Carolina was published in this Journal, for September, 1857.

By a private letter from Professor Guyot we learn that during last summer (1860) he has devoted two full months to further measurements in the south, in company with Messrs. Sandoz and Grand Pierre. The weather has been propitious and he has accomplished much work, having measured between one hundred and fifty and two hundred points in addition to those which were previously determined. He has extended his investigations as far as Georgia, and has seen the extremity of the Blue Ridge and the Unaka. It may now be affirmed with safety that the southern portion of the Alleghanies is better known so far as pertains to its hypsometry, than any other portion of the system. There is reason to hope that at an early day Professor Guyot will lay before the readers of this Journal, in detail, the results of his important and prolonged investigations; meanwhile the reader will be interested in the following partial summary of his observations in North Carolina.

These measurements sufficiently indicate the grand traits of structure of that loftiest portion of the Appalachian system. It may be seen that the Roan and Grand Father mountains are the two great pillars on both sides of the Northgate to the high mountain region of North Carolina, which extend between the two chains of the Blue Ridge on the east and the Iron and Smoky and Unaka mountains on the west. That gate is almost closed by the Big Yellow mountain. The group of the Black Mountain rises nearly isolated on one side in the interval between the two chains touching by a corner the high Pinnacle of the Blue Ridge, and overtowering all the neighboring chains by a thousand feet. In the large and comparatively deep basin of the French Broad Valley, the Blue Ridge is considerably depressed, while the western chain preserves its increasing height. Beyond the French Broad rises the most massive cluster of highlands, and of moun-

tain chains. Here the chain of the Great Smoky mountains which extends from the deep cut of the French Broad at Paint Rock, to that, not less remarkable, of the Little Tennessee, is the master chain of that region and of the whole Alleghany system. Though its highest summits are a few feet below the highest peaks of the Black Mountain, it presents on that extent of 65 miles a continuous series of high peaks, and an average elevation not to be found in any other district, and which give to it a greater importance in the geographical structure of that vast system of mountains. The gaps or depressions never fall below 5000 feet except towards the southwest and beyond Forney Ridge, and the number of peaks, the altitude of which exceeds 6000 feet, is indeed very large. On the opposite side, to the southeast, the Blue Ridge also rises again to a considerable height, in the stately mountains of the Great Hogback and Whiteside, which nearly reach 5000 feet, and keeps on in a series of peaks scarcely less elevated far beyond the boundary of Georgia.

Moreover the interior, between the Smoky mountains and the Blue Ridge, is filled with chains which offer peaks higher still than the latter. The compact and intricated cluster of high mountains, which form the almost unknown wilderness covering the southern portion of Haywood and Jackson counties, is remarkable by its massiveness and the number of lofty peaks which are crowded within a comparatively narrow space. The Cold mountain chain, which constitutes one of its main axes, shows a long series of broad tops, nearly all of which exceed 6000 feet. Near the south end, but west of it, not far from the head waters of the French Broad, the Pigeon and the Tuckaseegee waters, Mount Hardy raises its dark and broad head to the height of 6133. Still further, to the northwest, the group culminates in the Richland Balsam, 6425 feet, which divides the waters of the two main branches of Pigeon river and of the Caney fork of the Tuckaseegee. Amos Plott's Balsam, in the midst of the great Balsam chain, which runs in a parallel direction between the two main chains, measures 6278 feet. Considering therefore these great features of physical structure and the considerable elevation of the valleys which form the base of these high chains, we may say that this vast cluster of highlands between the French Broad and the Tuckaseegee rivers, is the culminating region of the great Appalachian system.

New Map of the Alleghany System, by Mr. E. Sandoz.—The measurements of Professor Guyot, just referred to, furnish important data for the correction as well as the completion of all existing maps of the regions which he has examined. These data, with the exception of those collected in the past summer, have been employed by Mr. Ernest Sandoz, a nephew of Prof. Guyot, and an accomplished draftsman, in the construc-

tion of a new map of the entire Alleghany chain, which has been published in the July number of Petermann's *Mittheilungen.* Mr. Sandoz had accompanied Mr. Guyot on many of his mountain expeditions and took the results with him to Gotha, where the chart was drawn and engraved under the direction of Dr. Petermann. To the editor of that excellent repository of geographical science, we are indebted for an early and proof impression of this map. As it is by far the most satisfactory chart of the Eastern portion of the United States in existence,—a request has been sent to Dr. Petermann to permit its republication in connection with this Journal, and there is reason to hope that at no distant day it may be laid before our readers, with a paper illustrative and explanatory of it, from the pen of Professor Guyot. The new edition when issued will contain some emendations derived from the more recent surveys to which allusion has been made.

The scale of the map is 1 : 6,000,000. Two detailed subordinate maps are printed on the same sheet with it, having a scale of 1 : 600,000, one of which gives the White Mts. of New Hampshire, the other the Black Mts. of North Carolina, both according to Mr. Guyot's measurements.

NARRATIVE OF A VOYAGE TO SPITZBERGEN IN THE YEAR 1613.—In the Transactions of the American Antiquarian Society, vol. iv. just published, S. F. Haven, Esq., the Society's Librarian, has edited with an introduction and notes, a Narrative of a Voyage to Spitzbergen made in the year 1613 at the charge of the English "Muscovey Company." Although this voyage is one of the series embraced in the celebrated collection of Purchas, commonly known as "His Pilgrimes," yet this account, which has been lying among the Manuscripts of the Antiquarian Society, has never before been printed. There is reason for believing, says Mr. Haven, that the Journal now first printed was from the pen of Robert Fotherby, whose name both as an author and as a skillful navigator is connected with two succeeding voyages.

"The expedition of 1613," he continues, "was fitted out with unusual care, and intrusted to the charge of some of the ablest men in the service. Besides the chief Captain, Benjamin Joseph, William Baffin and the author of our narrative, it was accompanied by Thos. Edge, who had already twice sailed to Spitzbergen. Purchas was indebted to Edge for the map of the coast inserted in his work, and also for a summary of Northern Discoveries which appears in the same volume. Baffin was attached to the ship of the commander of the fleet; and from that circumstance, apart from his personal reputation and the value of his scientific observations, his journal would naturally be the one selected for publication. The author of our account was in another vessel

often separated from the rest. He thus experienced a different series of incidents or observed the same from a different point of view."

At this period when Arctic explorations are attracting so much attention, the printing of this early record is peculiarly acceptable. The introduction and notes, with which the paper has been enriched by the pen of its learned editor, illustrate many interesting points pertaining to polar discoveries. The cuts which accompany this edition are fac-similes (except in their size which is half that of the originals) from some rude drawings which are attached to the Manuscript.

DR. ENGELMANN'S MEASUREMENT OF THE ELEVATION OF ST. LOUIS, ABOVE THE GULF OF MEXICO.—In the Transactions of the Academy of Science in St. Louis, vol. i, No. 4, 1860, there is an article by Dr. Geo. Engelmann on the elevation of St. Louis above the Gulf of Mexico, from which the following statements are derived.

"A knowledge of the exact altitude of St. Louis is important as an element in the physical geography of North America, not only for the reason that this city stands, so to say, in the centre of the great Mississippi Valley and not far from the confluence of the four great rivers, the Mississippi, the Missouri, the Illinois and the Ohio, but, also, because most of the hypsometrical measurements throughout the northern and western regions of this valley and into New Mexico and Utah, made by the different explorers during the last twenty years and more, by Nicollet, Fremont, Owen, Wislizenus, Emory, Stansbury, and several of the Pacific Railroad exploring expeditions, took the altitude of St. Louis as their starting point, and were based to a great extent on the barometrical observations of those explorers compared with mine.

"Mr. J. N. Nicollet was the first who ascertained the elevation of St. Louis as well as a great many points on the Mississippi and Missouri rivers, as he was the first to give us a physical geography of the Mississippi Valley, based on careful instrumental observation. He laid down an abstract of his labors in his Report on the Hydrographical Basin of the Upper Mississippi, in 1841, published by order of the U. S. Senate, after his death, in 1843. On pages 93–101 he gives a detailed account of the methods employed to obtain the desired results, and on pp. 122–125 is contained a most valuable table of geographical positions, distances, and altitudes."

In this table, the altitude of St. Louis is stated to be 382 feet from which two feet must be deducted to reduce it to low water-mark.

Dr. A. Wislizenus next calculated the elevation of St. Louis. His results are published in his Report on a Tour to Northern

Mexico, printed by the U. S. Senate in 1848. They place the altitude of the present low water-mark at 389 feet 6 inches.

Dr. Engelmann's recent calculations and measurements give the height of the low water-mark at St. Louis at 374 feet 4 inches, a few feet lower than either of his predecessors had estimated it.

The following table shows their comparative results for three different points measured.

Height of St. Louis above the Gulf, in English feet.

	Nicollet.	Wislizenus.	Engelmann.
Engelmann's Barometric Station,	486·5	496·0	480·9
City Directrix,	410·5	420·0	404·9
Low water-mark,	380·	389·5	374·4

"Intimately connected with the altitude of St. Louis and other points along our river is the question of the fall of the river and the velocity of its current. Nicollet's tables, mentioned above, give us the only data at present available for an approximative estimation of the fall of the Mississippi in its different sections. The following little table, calculated from these data, explains itself:

	Distances in miles.		Fall in feet.		Fall of the River in feet pr mile
	From point to point.	Total from St. Peter's.	From point to point.	Total from St. Peter's.	
Mouth of S. Peter's River to					
Prairie du Chien,	260	260	102	102	0·39
Rock Island,	210	470	114	216	0·54
Mouth of Desmoines,	128	598	84	300	0 65
St. Louis,	204	802	62	362	0·30
Mouth of Ohio,	174	976	58	420	0·33
Mouth of White River,	462	1438	122	542	0·26
Natchez,	348	1786	116	658	0·33
New Orleans,	302	2088	76	734	0·25
Mouths of Mississippi,	104	2192	10	744	0·09
Or in the great natural sections of its course:					
Mouth of St. Peter's River to					
Prairie du Chien,	260	260	102	102	0·39
Mouth of Desmoines,	338	598	198	300	0·59
New Orleans,	1490	2088	434	734	0·29
Mouths of Mississippi,	104	2192	10	744	0·09
Total average fall of the Mississippi from mouth of St. Peter's River to the Gulf,					0·34

"The Mississippi River has therefore an average fall of about 4 inches per mile; between St. Peter's and the Rapids, a little more; from the lower end of the Rapids to New Orleans, a little less; in the region of the Rapids, about 7 inches; and from New Orleans to the mouth, about 1 inch per mile. A further analysis of the tables shows the fall on both rapids to be 21 inches to the mile."

Dr. Engelmann then gives the following data, based, as he says, on his own "rather loose observations" respecting the velocity of the Mississippi. As he remarks on "the absence of all other information" on this point it seems proper for us to refer to Marr's Report of Observations at Memphis in 1849, and to Dr. Ellet's work, to the measurements of Riddell, Forshey, and Dickenson reported to the American Association, and to an article by Lyell in this Journal, [2], iii, pp. 36 and 118.

Date of Observation.		Height of River above low water.	Current per hour.	One mile in
1845.	Feb. 20,	5 feet,	3·00 miles,	20 minutes.
"	" 28,	10 "	3·50 "	17 "
1844.	Mar. 5,	15 "	4·00 "	15 "
"	Ap. 26,	20 "	5·00 "	12 "
1839.	May 27,	21 "	5·09 "	11¾ "
1837.	July 10,	27 "	5·55 "	10¾ "
1844.	May 19,	27 "	5·68 "	10½ "
"	Jun. 22,	35 "	6·25 "	9¾ "

KIEPERT'S NEUER HAND ATLAS, (Berlin, Reimer: N.Y., Westermann, 1860.)—This admirable work which has been for some years in progress is at last complete, presenting a collection of forty maps of different portions of the world. They are drawn with great beauty and skill and the whole work is at once attractive, convenient and trustworthy,—as might indeed be expected from the reputation which the author enjoys as a cartographer. Our limits do not admit of an extended criticism on the several maps, but we append a list for the information of those who may wish to purchase a complete general Atlas for every day use.

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EXPLORATION OF WESTERN BRITISH AMERICA.—In the last number of this Journal a condensed account was given of the expeditions sent out by the Canadian Government for the exploration of the Red river and Saskatchewan districts. Since then, we have received several important Canadian documents, referred to in that article. As they possess a permanent value we subjoin a notice of their contents.

1. Report from the Select Committee on the Hudson's Bay Company, together with the proceedings of the Committee, minutes of evidence, appendix and index. Ordered by the House of Commons to be printed 31st July and 11th August, 1857.

This public document presents in the usual style of the Investigations of a Committee of a House of Parliament, a vast amount of matter relating to the affairs of the Hudson's Bay Company and the regions under their control,—derived from the examination of Hon. J. Ross, Col. Lefroy, Dr. Rae, Sir Geo. Simpson, Sir John Richardson and other gentlemen. It is accompanied by three maps; 1, of the British and Russian possessions in North America; 2, of the aboriginal tribes of British America; and 3, of the Northwestern portion of Canada, Hudson Bay and Indian Territories.

2. Papers relating to the Affairs of British Columbia. Part I. Copies of Dispatches from the Secretary of State for the Colonies to the Governor of British Columbia, and from the Governor to the Secretary of State relative to the Government of the Colony; also Copies of the Act of Parliament to provide for the Government of British Columbia; Governor's Commissions and Instructions; Order in Council to provide for the Administration of Justice; and Instrument revoking so much of the Crown Grant of the 20th May, 1838 to the Hudson's Bay Company for exclusive trading with the Indians as relates to the Territories comprised within the Colony of British Columbia. Presented to both Houses of Parliament by Command of Her Majesty, 18th February, 1859. London, 1859.

This blue book relating chiefly to Fraser's river and the gold discoveries has an outline sketch of the western portion of British America showing the different routes of communication, across the country to the mouth of the river.

3. Northwest Territory.—Reports of Progress together with a preliminary and general Report on the Assiniboine and Saskatchewan exploring Expedition, made under instructions from the principal Secretary, Canada. By HENRY YOULE HIND, M.A. in charge of the Expedition. Toronto, 1859.

4. Report on the exploration of the Country between Lake Superior and Red River Settlement and between the latter place and Assiniboine and Saskatchewan. By S. J. DAWSON, Esq., C.E. Printed by Order of the Legislative Assembly. Toronto, 1859.

A particular account of these expeditions was given in the last number of this Journal, p. 218.

5. Geological Survey of Canada. Report of Progress for the year 1858. Montreal, 1859.

6. Map of the Northwest part of Canada, Indian Territories and Hudson's Bay; compiled and drawn by THOS. DIVINE, Provincial Land Surveyor and Draftsman, by order of Hon. JOS. CAUCHON, Commissioner of Crown Lands, Toronto, March, 1857. Published by S. Derbeshire and G. Desbarats.

This is a map in outline, geologically colored, of the region above mentioned, extending as far north as 75° north latitude and as far south as 45° north latitude. Being made previous to the recent exploring expeditions it is now behind our present knowledge. The authorities on which it is based are J. Arrowsmith, A. Mackenzie, D. Thompson, the Admiralty and Coast Survey Charts, Pacific Rail Road Survey Reports. Sir Geo. Simpson, Governor of the Hudson's Bay Company, Sir Wm. Logan and others are also quoted. The isothermal lines are given according to Dove and Blodget.

7. Government Map of Canada from Red River to the Gulf of St. Lawrence, compiled by THOMAS DEVINE, Head of Surveys, Upper Canada Branch, Crown Land Department. November, 1859.

This map in three sheets, is limited to Upper and Lower Canada, the counties of which are distinctly indicated by color, and the more detailed topography is also clearly given. It is clearly drawn, and presents in its margin a variety of useful information respecting the resources of Canada.

DR. BARTH IN ASIA MINOR.—Dr. Henry Barth, the celebrated traveller in Africa, has published in a Supplement to Petermann's Mittheilungen an account of a journey which he made in 1857–1858 from Trapezund, through the interior of Asia Minor, by way of Tokat, Amássia, Yûsgad, Cesarea and Angora to Constantinople. His attention was directed to the physical structure of the country and also to the remains of ancient art. The archaeological inquiries which he made will hereafter be made public, in detail,—though many of the more important facts are brought out in Petermann with illustrative wood cuts. A sketch of his journey is given on an accompanying chart with plans of Tókat, Amássia, Kara-hissár and Kyr-schehr.

D. C. G.

ART. XXXVII.—*Further Remarks on Numerical Relations between Equivalents*; by M. CAREY LEA.

IN papers on this subject published in the January and May numbers of this Journal for the present year, I endeavored to show that a large number of so-called elements could be arranged in seriated groups, the members in each series differing from each other by a common quantity, in most cases the number 44 or one approximating to it. I endeavored to show that not only were these groups natural groups, but that the chemical properties of the members of each group corresponded in many cases with their position in it. These observations seemed to favor the view at present gradually gaining ground, that those bodies which we have as yet failed to decompose, we have not proved to be elementary.

An interesting and elaborate paper by Gustav Tschermak, published in the Proceedings of the Academy of Science of Vienna, and extracted in an abridged form in Knop's Centralblatt, (July 4, 1860,) on the subject of the law of volumes of liquid chemical compounds affords a support to the views above expressed, from a new source. The author therein shows that many of the substances usually classed as elements comport themselves in the physical properties exhibited by their combinations as compound bodies, and that it is possible from these physical properties to determine (hypothetically) the number of "physical" or absolute atoms which he supposes to be contained in a chemical atom of such body or pseudo-element. He endeavors to show that it is possible to calculate the specific gravity of a liquid from its atomic weight and the number of simple (chemical) atoms in its compound molecule, as data, but that the results lead to the immediate inference that each chemical atom contains with few exceptions several physical atoms.

For particulars of his theory I must refer to the original paper, but some of his results are as follows:

	Physical atoms to each chemical atom.*
O (O=16)	2
S (S=32)	4
F	2
Cl	4
N	2
P	4
As	5
Sb	6

* These numbers are taken from the table of mean numbers, p. 508 of Centralblatt and are those subsequently used by the author for determining the "physical atomic weights"$=\frac{m}{n}$.

If now we arrange the first six of these substances in parallel series we shall find

		Atomic weight.		Physical atoms.
Sulphur,		32		4
Oxygen,		16		2
	difference,	16	difference,	2
Chlorine,		35·5		4
Fluorine,		19		2
	difference,	16·5	difference,	2
Phosphorus,		31		4
Nitrogen,		14		2
	difference,	17	difference,	2

Thus a common difference in each case amounting to 16–17 corresponds with a difference of two of the physical atoms into which Tschermak divides the chemical atoms.

If now we put O=2o, Cl=2cl, P=2p, the approximate difference between S and O, Cl and F, &c., (16–17)=$2\triangle'$, the difference (48) between S and Se=$\triangle''$ and the difference (44–45) between the terms of the nitrogen series =$\triangle$, we can express the whole of three important series in terms of these six quantities, so that at one and the same time both the numerical value of the atomic weights and the number of Tschermak's physical atoms shall be correctly expressed.

	Symbols.	At. weights.	Physical atoms.
A. Oxygen group.			
Oxygen,	o_2	16	2
Sulphur,	$o_2\triangle'_2$	32	4
Selenium,	$o_2\triangle'_2\triangle''$	80	5
Tellurium,	$o_2\triangle'_2\triangle''_2$	128	6
B. Chlorine group.			
Fluorine,	fl_2	19	2
Chlorine,	$fl_2\triangle'_2$	35·5	4
Bromine,	$fl_2\triangle'_2\triangle$	80	5
Iodine,	$fl_2\triangle'_2\triangle_2$	127	6
C. Nitrogen group.			
Nitrogen,	n_2	14	2
Phosphorus,	$n_2\triangle'_2$	31	4
Arsenic,	$n_2\triangle'_2\triangle$	75	5
Antimony,	$n_2\triangle'_2\triangle_2$	120·3	6

In which table the number of radicals by which the chemical atom of each body is expressed, corresponds with the number of Tschermak's physical atoms, while their numerical value is equal to the atomic weight of the body.

Thus tellurium $o_2\triangle'_2\triangle''_2$ would have two each of three radicals, in all six, agreeing with the number of physical atoms as-

signed to it, while their value $2 \times 8 + 2 \times 8 + 2 \times 48 = 128$, at. wt. of tellurium.

These observations of Tschermak, taken in connection with the numerical relations which exist between atomic weights, give rise to very interesting results, and if the conclusions which he arrives at from his experiments should be confirmed, they cannot fail to exercise a very important influence on the progress of chemical science.

Philadelphia, August 28th, 1860.

ART. XXXVIII.—*On the Production of Ethylamine by Reactions of the Oxy-Ethers*; by M. CAREY LEA.

[Read before the Am. Assoc. for the Adv. of Sci. at Newport, August, 1860.]

WHILE engaged in making a series of experiments on this subject I met with the paper of Juncadella* and the observations of De Clermont† on the same subject. Finding that the subject had less novelty than I supposed I merely offer here one or two of the results which I have obtained.

Nitrate of ethyl C_4H_5O, NO_5 heated in sealed tubes with chlorid of mercurammonium $\left.\begin{matrix}H_3\\Hg\end{matrix}\right\} NCl$ for many hours in the water bath did not appear to react upon it. Kept for some time in a boiling saturated solution of chlorid of calcium the tubes although extremely thick green glass-combustion-tube of small calibre was used, exploded with great violence, shattering the vessel in which they were contained, although they had been wrapped in strong cloth.

Nitrate of ethyl heated in a sealed tube with chlorid of zinc-ammonium $\left.\begin{matrix}H_3\\Zn\end{matrix}\right\} NCl$ in the water bath, does not appear to act upon it.

Nitrate of ethyl heated in the water bath in a sealed tube with carbamate of ammonia $NH_4O, NH_2C_2O_3$ dissolves the salt. On cooling radiated crystals form. The contents of the tube evaporated to dryness with excess of chlorhydric acid, and then exhausted with ether to which a few drops of strong alcohol have been added, yielded a solution which gave a chamois colored precipitate with bichlorid of platinum, consisting of chloroplatinate of ethylamine.

·1185 gm. substance gave ·0465 metallic platinum corresponding to 39·25 per cent; theory requires 39·29

The product was but small. Probably portions remained undissolved by the ether. No doubt portions of di- and triethylamine are also formed in the above reaction, in the same manner as in those of the halogen ethers with ammonia.

Philadelphia, July 11, 1860.

* Rép de Chimie pure, Tome 1, 273. † Ibid., 274.

ART. XXXIX.—*On the Optical Properties of the Picrate of Manganese;* by M. CAREY LEA, Philadelphia.

[Read before the Am. Assoc. for the Adv. of Sci. at Newport, August, 1860.]

BREWSTER and HAIDINGER have described a remarkable property possessed by certain crystalline surfaces, of reflecting, besides the ray normally polarized in the plane of incidence and reflection, another ray, polarized perpendicularly to that plane, and differing from the former in being colored, a property rendered more conspicuous by the fact that the color of the ray so polarized abnormally is either complementary to, or at least quite distinct from the color of the crystal itself.

I find that this property is possessed to a remarkable degree by the picrate of manganese. This salt crystallizes in large and beautiful transparent right rhombic prisms, sometimes amber-yellow, sometimes aurora-red, exhibiting generally the combination of principal prism, and macrodiagonal, brachydiagonal and principal end planes. In describing this substance in a paper on picric acid and the picrates,* I mentioned that in a great number of specimens examined, no planes except those parallel with or perpendicular to the principal axis had been met with. Since then I have obtained in several crystallizations specimens exhibiting a brachydiagonal doma, but this appears to be rather unusual.

The optical properties of this salt are very interesting. It exhibits a beautiful dichroism. If the crystal be viewed by light transmitted in the direction of its principal axis, it appears of a pale straw color, in any other direction, rich aurora-red in some specimens, in others salmon color. A doubly refracting achromatised prism gives images of these two colors, except the light be transmitted along the principal axis of the crystal of picrate, in which case both are pale straw color.

But it also possesses in a high degree the property of reflecting two oppositely polarized beams, and the great size of the crystals in which it may readily be obtained, renders it peculiarly fitted for optical examination. If one of these crystals be viewed by reflected light while it is held with its principal axis lying in the plane of incidence and reflection, the reflected light is found to be not pure white, but to have a purple shade. Examined with a rhombohedron or an achromatised prism of Iceland spar, having its principal axis in the plane of incidence and reflection, the ordinary image is white as usual, while the extraordinary is of a fine purple color, the phemenon having the greatest distinctness when the light is incident at the angle of maximum polarization.

* This Journal, Nov., 1858.

The experiment may be varied and the purple light beautifully seen without the use of a doubly refracting prism by allowing only light polarized perpendicularly to the plane of incidence to fall on the crystal; in this case the surface of the crystal appears rich deep purple, no white light reaching the eye.

This property is not possessed by all the planes of the crystal, but is limited to the principal prism and brachy- and macrodiagonal end planes, in other words to the planes parallel with the principal axis of the crystal. The brachydiagonal doma and $0P$ planes do not possess it. Nor is it exhibited by the first mentioned planes, when the crystal is turned with its prismatic axis at right angles to the plane of incidence.

All specimens of picrate of manganese do not possess this property to an equal extent. The crystals vary considerably in color, and those which are full red exhibit it more strongly than the amber colored. Picric acid boiled with aqueous solution of cyanhydroferric acid and saturated with carbonate of manganese gives crystals of a rich deep color, which exhibit the purple polarized beam particularly well.

These properties are not possessed by the manganese salt alone, but also by the picrates of potash and ammonia, (especially when crystallized by very slow spontaneous evaporation in prisms of sufficient size) and the picrates of cadmium and peroxyd of iron —with this difference however, that while the prismatic axis of the crystal in the case of the cadmium and manganese salts must be in the plane of incidence, in the alcaline salts it must be perpendicular to that plane. As they all crystallize in the right rhombic system, it is probable that either the alcaline salts on the one hand, or the manganese and cadmium or the other, are prismatically elongated in the direction of a secondary axis.

It is convenient that distinct phenomena should have distinct names, and none appears to have been assigned to this. Brewster speaks of it as a "property of light," and Haidinger uses the word "Schiller" for it. The terms dichroism, trichroism and pleiochroism are limited to properties of transmitted light. I therefore suggest for that here in question the name *catachroism*, using the preposition κατα in the same sense as in the word κατοπτριζω, to reflect, (as a polished surface,) applying it to express the property of reflecting two beams, one normally polarized in the plane of incidence and the other polarized in a plane perpendicular to it.

The chromatic properties exhibited by the picrates of ammonia and potash are very remarkable in their variety. Their crystals possess:—

1st. The well known play of red and green light. If a little very dilute solution of pure picrate of potash be spontaneously

evaporated in a hemispherical porcelain basin, so as to form a net work of extremely slender needles, and these be viewed by gas light, the play of colors is singularly brilliant.

2d. Dichroism. When by spontaneous evaporation of large quantities of solution of potash, or better, of ammonia salt, transparent prisms of $\frac{1}{12}$ to $\frac{1}{16}$ inch diameter are obtained, these, viewed with a doubly refracting prism by transmitted light give two images, one pale straw color, and the other deep brownish red.

3d. The above described property of catachroism, or reflection in the plane of incidence of oppositely polarized beams.

Philadelphia, August, 1860.

ART. XL.—*On our inability from the Retinal impression alone to determine which Retina is impressed;* by Prof. WILLIAM B. ROGERS.

[Read before the Am. Assoc. for the Advancement of Sci., at Newport, Aug. 1860.]

ALTHOUGH on first view it might be supposed that an impression made in either eye must necessarily be accompanied by a mental reference to the particular organ impressed, it will be seen from the following simple experiments that the impression of itself is not essentially suggestive of the special retinal surface on which it is received.

Exp. 1. Let a short tube of black pasteboard $\frac{1}{8}$ inch in diameter be fixed in a hole in the centre of a large sheet of the same material. Hold the sheet a few inches before the face of a second person and between him and a bright window, moving it to and fro until the bright circular aperture of the tube is brought directly in front of one of the eyes, suppose the left eye; and let him fix his attention upon the sky or cloud to which the tube is directed. He will feel as if the impression or image of the hole *belongs equally to both eyes* and will be unable to determine which of them really receives the light.

On moving the aperture towards the right, or nearer the nose, but not so far as to be out of the view of the left eye, or to be visible by the right, the observer will imagine that it is now in front of the right eye and chiefly seen by it. Shifting it still further in the same direction, until it is brought within view of the right eye but not fairly in front of it, it will appear as if placed before the left eye, and by an additional motion bringing it fairly in front of the right eye it will seem to be equally before both eyes or to be in the medial line between them.

Like effects may be observed by using a half sheet of rather stiff foolscap with a large pin hole in the centre. Bending this over the face and moving it until the hole is in front of one of

the eyes, the same uncertainty and contradiction will be produced as in the preceding experiment.

Exp. 2. Similar results may be obtained by rolling half a sheet of letter paper into a tube of about one inch in diameter, and holding it before and a little in advance of one eye while both are directed to a white wall some feet distant. Keeping the view *fixed* upon the wall there will be seen upon its surface a circular image of the remote aperture of the tube. This as we look intently at it will appear as if seen equally by both eyes, occupying a midway position between them. If now the eyes be converged to some point nearer than the end of the tube the circular image will appear against the side of the tube giving the impression that it is seen by the eye which is remote from the tube and is at the same time directed toward the outside. For the complete success of this experiment the wall should be only moderately bright, and but little light should fall on the exterior of the tube next the uncovered eye.

Exp. 3. Let two tubes of stiff paper each one inch in diameter and six inches long be held close to the two eyes in a converging direction so that the outer ends may touch each other. Then directing the view through them to a white wall at a short distance, the observer will see the two tubes as one, with a single circular opening clearly marked out on the wall. If now a small object as the end of the little finger be brought near and in front of one of the tubes, it will take its place within this circle and will seem to be equally an object of vision to both eyes, so that the observer will be wholly unable to decide before which eye it is actually placed.

Let the observer next direct his view to a very remote object, as the sky, seen through the window, still retaining the previous adjustment of the tubes. He will now see two circles, continuing separate as long as he keeps his eyes fixed on the distant surface; and if the finger be held up as before in front of one of the tubes it will appear within the circle which is in front of *the other eye;* thus causing the impression on the right eye to be apparently transposed to the left, and vice versa.

Exp. 4. Fasten a small disc of white paper on a slip of black pasteboard of the size suitable for a stereoscope, and place this in the instrument so that the white spot shall be centrally in front of one of the glasses.

To a person not aware of the position of the spot it will appear in the stereoscope as if equally in view to both eyes and he will be entirely unable to decide on which retina its picture is impressed. Indeed properly considered the spot does not appear directly in front of either eye but is seen at the intersection of the optic axes, in the medial or binocular direction between the two.

Let the spot be now moved toward the right side but still within the range of the left eye and it will seem to be before the right eye rather than the left. Shift it into the right compartment but not far from the dividing line and it will appear as if seen chiefly by the left eye, and finally bring it to the middle of the right compartment and it will seem as at first to belong equally to both eyes.*

Referring to the results observed in the above experiments when the object is directly in front of either eye it may be concluded that the mere retinal impression, on either eye is unaccompanied by any consciousness of the special surface impressed, and that the formation of the visual perception appertains to that part of the optical apparatus near or within the brain, *which belongs in common to both eyes.*

These observations show moreover that the *perceived direction is just as truly normal to the central part of the retina which has received no light as to that of the retina on which the white spot has been painted.* Indeed as before indicated, it is normal to neither, but is felt to be in the middle line between the two, that is, in the binocular direction. It need scarcely be added that this conclusion is at variance with the law of visible direction maintained by Brewster, which requires that the apparent direction of an object shall in all cases be normal to the part of the retina impressed.

The reference of the object in certain cases above noticed, (parts of 1, 2 and 4) to *one eye chiefly*, and that the eye from which *it is actually hidden*, is accounted for by the direction in which the other eye receives the light. As this direction, in the case of the left eye for instance, would be decidedly toward the field of view of the right eye, it would at once suggest the place of the object as somewhere before that eye, and so when the object is actually before the right eye, but in a position towards the left, it would excite the idea of an object somewhere before the left eye. As the retinal picture alone gives no indication of the particular eye in which it is formed, but only excites a visual consciousness common to both, the object in these cases will seem to be visible by both eyes but chiefly by that before which the suggestion just mentioned would naturally place it.

A like explanation applies to the transposition observed in Exp. 3, when the view is directed to a distance through the converging tubes. Here the false visual reference of the finger depends on the fact that the circle in front of either eye is sugges-

* The effect here described is one of a series of phenomena which Dr. O. W. Holmes attributes to an *actual transfer* of impressions from one eye to the other, and which he proposes to explain by the hypothesis of reflex vision. Proc. Am. Acad. Arts and Sciences, Feb. 1860.

tive, merely by its position, of a *special vision by that eye*, while from the conditions of the experiment these circles are in fact reversed in their places as compared with the tubes and eyes to which they appertain.

We have seen in the above experiments that when an object is presented to one eye without any accompanying circumstances leading us to refer the visual act specially to this or to the other eye we have a consciousness of seeing it equally with both eyes. *The same result occurs when separate objects are presented to the two eyes*, provided as before, extraneous sources of suggestion are excluded.

Exp. 5. Thus if we place on the black slide of the stereoscope two spots, differing either in shape or color, one before each eye, we perceive them both in the middle or binocular direction, each seemingly visible in an equal degree to both eyes, the one being seen through or upon the other according to the fitful attention or suggestion of the moment. A pleasing modification of this experiment is made by using two unequal white spots on the black slide and interposing a green or other colored glass between one of them and the lens. The spot which appears colored will give as strongly the impression of being seen by both eyes as the white one, in spite of our knowledge of the position of the colored glass.

Even in cases where the two objects are wholly unlike, and at very different distances from the eyes by which they are severally regarded, this feeling of a common or united visual act in regard to each of them is often easily recognized. Of this we have a ready illustration in the familiar experiment on ocular parallax in which a distant object, hidden from one eye by an interposed finger or pencil, is seen through or behind the pencil when both eyes are directed towards the distant object.

Exp. 6. To observe this effect satisfactorily it is well to make the experiment in an apartment in which a single small lamp is placed at some distance from the spot on which we stand. Looking intently at the lamp, we bring the pencil before the face in such position as to give us an image on each side of the lamp, and then move the pencil toward the right until its left hand image seems to coincide in direction and position with the lamp, which appears to shine through or to partially replace it. As we continue to look thus at the lamp, we have a clear impression that both lamp and pencil are equally visible to both eyes, and without some consideration of the previous adjustment and motions we are unable to determine which is actually visible to the right and which to the left eye.

The same experiment furnishes also an incidental illustration of the principle of *transposed* visual reference before alluded to.

If, while the above adjustment is maintained, we contemplate the other image of the pencil situated some distance to the right of the lamp, and endeavor to decide, from the mere visual impression, to which eye it appertains, we almost unfailingly refer it to the right eye as that which most nearly fronts it, although obviously it belongs to the other, as will be found at once on closing either eye.

Where the eyes are externally very sensitive, any strong illumination of one as compared with the other will interfere with the effect above described by referring the impression specially to the eye thus unduly excited. In snch cases the observation is best made in a moderately lighted room by interposing the pencil between the eye and a vertical stripe on the wall.

Exp. 7. Recurring to Exp. 2, in which with a tube in front of one eye we perceive a bright circle on the wall in the medial direction, we may obtain a pleasing illustration of the point now under consideration by bringing a dark card or book or even the hand between the uncovered eye and the wall. The spot instead of being intercepted will appear as a perforation in the opaque screen.

Here as in the case of the pencil and lamp, the bright circle and the screen are both optically referred to the intersection of the two lines of view. But the luminous circle almost or entirely obliterates the corresponding part of the screen. As the full view of the screen and its connections continually remind us that it is in front of the uncovered eye, we are led to refer the luminous circle seen as coincident with a part of it, to the same eye, and thus to believe that *we are looking through the screen with that eye.* It is however not difficult, by intently regarding the luminous circle, so to counteract the force of this extraneous suggestion as to feel even in this case as if the circle were equally in view to both eyes.

These considerations explain very simply the experiment of the pseudo-diascope described by Mr. Ward of Manchester, which like several of those above mentioned is but an instance of the old observation of Da Vinci, that when we see behind a small opaque object presented near the eyes "it becomes as it were transparent." In making this experiment with a tube of paper supported between the thumb and forefinger of the left hand and held before the right eye so that the back of the hand may be some inches in advance of the left eye, it will be noticed that the effect varies with the amount of convergence of the eyes and that the bright perforation in the hand *may or may not* be referred to the left eye according to the force of the accesscry suggestions or the intentness with which we fix our gaze upon the distant spot to which the axes are converged.

In concluding, it may be remarked that the experiments which have been described are for the most part too obvious and familiar to have merited such a special notice but for the peculiar and in some respects new interpretation which they have offered of many visual phenomena. Considered in this relation we are I think entitled to conclude from them:—

First that the retinal impression of an object presented directly to either eye is accompanied by the feeling of a united visual act, and of itself gives no indication of the particular eye impressed; and:—

Second, that the reference of the impression to one eye rather than the other is the result of collateral *suggestion*, which may either locate the image in the eye that actually receives it, or may transpose it seemingly to the other, according to the particular conditions of the observation.

ART. XLI.—*Correspondence of J. Nicklès, of Nancy, France.*

Physical Chemistry.—Polarized Light employed as a Reagent.—BIOT, who entered his 86th year on the 21st of April last, and who continues to labor with the ardor of youth, has just published a résume of his complete works on circular polarization, applied to the study of chemical species. These researches, which he has greatly promoted, have often occupied us and daily gained more importance. Biot has given us the following account of the manner in which he was led to the discovery of the fundamental fact which has given point to all his labors in this field. He says:—

"Accident, that great promoter of physical novelties, gave me in 1815 the first hint in some experiments made for an entirely different object.

Wishing to study the changes of a polarized ray, when it passes very obliquely through thin plates of sulphate of lime, I had enclosed such a plate in a large metallic tube, about 8 centimetres long terminated by parallel faces of glass, and filled with colorless essence of turpentine. This was attached to a metallic rod projecting out transversely, which turned on its axis around the centre of a graduated circle, allowing me to change at pleasure the inclination of the interior incidence of the polarized ray. Having thus brought the plane of the plate in the direction of the ray itself, so as to fix the point of departure of their inclinations, the principal section of the analyzing prism being previously placed in the plane of the primitive polarization so as not to disturb it, I perceived in that part of the field of view occupied by the essence an indistinct, extraordinary image, of a blue color, caused by the interposed essence.

Water and alcohol previously introduced into the same tube, produced nothing similar. The effect observed, was not therefore due to the interposition of the essence, as a *liquid* mass. It must necessarily result from some physical property, peculiar to the substance which produced it.

I hence inferred that there was a deviation from the plane of polarization, to the right or the left, as in plates of quartz cut perpendicular to the axis. Revolving the analyzing prism successively in these two directions, towards the left, the blue image gradually grew fainter, until at a few degrees distance, it disappeared entirely, and reappeared beyond colored red, which I had found to be a distinctive character of rotary phenomena produced by plates of left-handed quartz. Hence I immediately concluded that a phenomenon of the same kind, was here presented except that it was produced by the successive actions of similar particles of the essence."

The principal fact, its molecular character, and the general consequences which are deduced from it, were made known to the Academy of Sciences, the 23d of October, 1819, and were published in a few days after in the Bulletin of the Philomathic Society of the same year, p. 190.

Since then Biot has most perseveringly prosecuted these general studies and labored to have chemists adopt them as a means of studying the intimate constitution of bodies without decomposing them, inasmuch as chemical analysis is applied to the study of those bodies only after they have ceased to exist: he explains the difficulties he has surmounted in persuading chemists to adopt this method. It is credible that these difficulties would not have been overcome, if the author of circular polarization had been an obscure *savan*—just as the value of the beautiful discoveries of Pasteur, would not yet have been recognized had not Biot been their interpreter. Biot closed his recapitulation of the principal discoveries of which the circular rotatory power of molecules had been alike the occasion and the instrument by the reflection, if his investigations had left undiscovered other facts of a kindred nature, no one could regret the involuntary omission more than himself.

On the existence of new simple bodies.—The beautiful labors which Bunsen and Kirchhoff have recently published (see page 415) on this interesting topic, the great advantage which they derive from the wonderful and peculiar action of the different metals upon the solar spectrum, the use made of it for qualitative chemical analysis and which they propose to make of it, in investigating the nature of the light of the sun and the fixed stars,—all these and many other questions raised by their memoir, have recalled attention to a research published ten years ago by Leon Foucault on the rays of the electrical spectrum. This physicist first proposed the use of points of gas carbon to form the voltaic arc.*

In studying this arc in 1847–48, he discovered that the ray D of the electrical spectrum coincides with that of the solar spectrum, and hence we may produce a superposition of two spectra, by throwing on the voltaic arc a solar image formed by a convex lens. When metals, which produce the ray D only feebly, as iron or copper, are used as poles, remarkable intensity can be secured, by touching them with a salt of potash, soda, or lime.

Foucault further adds :—"Before inferring anything from the constant presence of the ray D we must ascertain if its appearance does not indicate the same material mingled with all our conductors. Still this phenom-

* L'Institut, Feb'y, 1849; see also this Journal, [2], xxix, 424.

enon urges us to study the spectra of the stars—for if fortunately the same ray were there found, stellary astronomy would be advanced."

We recall in fact, that relying on their own observations, Messrs. B. and K. have admitted the presence of potassa and soda in the sunbeam, (*Journal de Pharmacie et de Chimie*, May, 1860, and this Journal, xxix, p. 424,) in the complete account they lately gave in Poggendorff's Annals, T. cxl, p. 161. These savans have shown how sensitive this characteristic is that even infinitesimal traces of potassa, soda and lithia can be recognized in the rays they produce and these characteristics continue even when the bases are reunited.

We will not dwell further upon this important research which is elsewhere presented in this Journal: but we should point out another direction of these researches, that which relates to the discovery of new simple bodies. Messrs. Bunsen and Kirchhoff made known lately the probable existence of a fourth alkaline metal, placed in relation to the rays of its spectrum immediately after lithium and preceding strontium.

It is thought that this process will determine the existence of the new metal dianium announced by M. Kobell (this volume, July, 1860, p. 123) and whose reality is contested by H. Rose.

Catalysis and Contact Actions.—The class of phenomena in chemico-physical research which for the want of a better term we distinguish as above, has been enriched by the discoveries of H. Loewel,* whose researches have remained hitherto too little known, but which demand attention from the very original facts they have brought to light; which are the fruits of several years of observation.

His researches on supersaturated saline solutions.—It is well known that a supersaturated solution of crystallized sulphate of soda exposed to the air crystallizes suddenly when touched by a glass rod, but that it does not crystallize when this rod is heated to 100° C. This fact is connected with many investigations which Loewel followed for a part of his life, and which are the subject of a critical analysis published by Hirn, a pupil of Loewel, who is well known by his interesting researches on heat.

Hirn's analysis of his teacher's results has rather the character of a dissertation filled with new facts, which although they may have been disputed, have yet remained so hidden in the diffuse memoirs of Loewel that they appear, as it were, now for the first time. As the research is too long to reproduce here, we content ourselves with citations from it.

* Poggendorff's Biographical Dictionary speaks of this Author thus: "Loewel, Henri, Director of the chemical works at Choisy-Le-Roi, from 1815 to 1819, since then colorist at Münster (Upper Rhine), born on the 29th of Sept. 1795 at Münster, died 5th of Sept. 1856, at Colmar: his published researches are:—

Upon some salts of chromium, *Annales de Chim. et de Phys.* (Ser. iii, T. xiv, 1845.)

Upon the chlorids of chromium (ib. xv, 1845.)

On the supersaturation of saline solutions (ib., xxix, 1850, xxxiii, 1851, xxxvii, 1853, xliii, 1855, xliv, 1855, xlix, 1857.)

Upon the action of zinc and iron upon the solution of the salts of sesquioxyd of chromium (ib., xl, 1854).

On the solubility of the carbonate of soda (ib. xliv, 1855).

On the chlorohydrate and sulphate of chromium (*Comptes Rendus*, xx, 1845.) See also *Le Recueil du Journal de Pharmacie et de Chimie*, which has also published these memoirs, a little earlier than the dates above quoted."

Reverting to the experiment with a supersaturated solution of sulphate of soda, we see it is the *air* adherent to the glass rod which determines the act of crystallization, since if the rod is heated to 100° C. no such result follows. Is it then the air which suffices for this action or some peculiar quality contained in the air? The latter supposition seems the most probable since it is not caused by air which has been filtered through cotton contained in a tube, nor by air which has passed through a properly arranged series of flasks connected by tubes of glass. Air thus agitated or heated by friction, may be brought in contact with the supersaturated solution, under the form of a continued current, without determining the crystallization, which commences immediately in presence of normal air. Loewel attributes the modification produced by the air to the friction produced in his mode of experiment, and a recent experiment of Hirn proves that it is so.

The air thus rendered passive by Loewel is called *adynamic* air. Hirn has observed that the air is rendered completely adynamic when it escapes after compression in the form of a jet from the receiver in which it was confined. After this compression it can be directed with impunity into a solution of sulphate of soda saturated by heat in a closed vessel. On the contrary, the solution solidifies instantaneously if by the same tube, and without any derangement of the apparatus, some bubbles of ordinary air are allowed to pass into the solution. Here then, is an action purely mechanical which replaces the action of heat, a remarkable example of the correlation of force which raises a crowd of questions and which leads to the inquiry if an identical composition of the air should always have an identical action upon a living being; if air rendered adynamic by a storm is not found in different conditions in relation to organized beings than air long undisturbed.

This brings to mind that Schrœder and Busch have shown that fermentation is not caused by air filtered through cotton, and we now ask if the air rendered adynamic by the process of Hirn will not possess still more passivity.

It is an argument more in favor of this theory now held by the advocates of spontaneous generation to know that it is not by germs of infusoria suspended in the air that fermentation or putrefaction is carried on. These experiments appear to us to touch questions of the greatest importance in the sciences of observation, as well as others relating to the most interesting considerations in cosmogony.

Empiricism.—Application of the Physical Sciences to Medicine.—A discussion which has recently taken place in the Academy of Medicine on the action of iron used as a medicine has made known to us this unexpected fact that there are physicians who deny any influence exercised by medicines in virtue of their chemical properties, and who think that the physical and chemical actions of the animal economy differ entirely from those which are observed in the vegetable kingdom.

At the head of this retrograde school (which ignores the progress made by physics and chemistry in the last eighty years and to whom organized beings are composed of a material which is not subject to the general laws of matter which composes the universe) appears a physician celebrated more than the rest, Dr. Trousseau, who raising the banner of

vitalism has declared that chemical laws explain nothing when used in relation to man, and that medicinal agents act by unknown and very different means from those which chemists suppose. Space does not allow us to notice the reply made at the same sitting by another physician, Poggiale, who is also somewhat of a chemist. But we shall be asked what is the precise meaning of *vitalism?* *Vitalism* is a force in the category of what has been called catalytic force; this is a word which conceals our ignorance and which is evidently an obstacle to progress. This recalls that saying of Liebig, "If we allow forces to be created, investigations become useless and it will be impossible to arrive at the knowledge of truth." Vital force is then entirely for those physicians who ignore the first notions of physics or chemistry and think all has been said when they have installed this senseless word in place of an organic fact which ranks under the laws of mechanics, physics or chemistry. Vital force is insufficient to explain how it happens that a large number of substances, such as sugar, tartaric and malic acids, sulphur, sulphurets, salicine, &c., &c., undergo in the animal economy the same changes as when subjected to chemical action.

When we remember that slight compression of a muscle suffices to develop heat, and that its contraction evolves electricity, that in order to establish chemical action it suffices to place two heterogeneous bodies in contact—one is surprised that medical men should seek to explain the phenomena of life by "vital force"; as if the material of our bodies was exempted from the laws that regulate matter, as if what they call vital laws could interfere with the play of physical, mechanical, or chemical laws.

The discussion is not yet closed. It is still continued in the Medical Journals, some of which like M. Trousseau are vitalists in pathology and empirics in the domain of therapeutics.

*Electro-Magnets and Magnetic Adhesion.**—We have on several occasions given in this Journal the progress of our researches upon magnetic adhesion, as well as the laws which regulate electro-magnets. At the request of several physicists and mechanics we have published the whole of our researches upon this subject, and a small octavo volume under the above title is the result. For the two electro-magnets, the rectilinear and the horse-shoe which were known in 1850, we must now substitute a larger number (several hundred) differing from each other by determinate properties. All these new electro-magnets which are described in this book, or whose existence has been foreseen, have at first rendered our task very difficult, for we were obliged to give names to them in order to facilitate their study. To give them such names as '*horse-shoe*' only rendered their study more difficult, we therefore preferred to group them under a systematic nomenclature according to the principle of their natural classification.

We divide the electro-magnets into two great classes according to their form, these are:—

1. Branched Electro-magnets.
2. Disk-shaped Electro-magnets.

* *Les Electro-aimants et l'adhérence magnétique.*

These classes are subdivided into families which rank according to the number of branches or disks of which they are composed; thus the rectilinear electro-magnet having only one branch will form the first family. The electro-magnet with two branches will serve as a type of the second family which will be that of bifurcated magnets, and the trifurcated electro-magnets or those with three branches form the third family; and finally the fourth family is composed of multifurcated electro-magnets, i. e., those with more than three branches. The families of electro-magnets stop here; there are consequently no *quinto*, *sexto* . . . *n* furcated electro-magnets, experience having shown that the properties of electro-magnets with more than three branches are very much the same, one new branch adding no new property.

The same method is followed with disk-shaped electro-magnets whose name is derived from *dromos*, course, in order to distinguish their most characteristic property, that of turning or revolving. These electro-magnets are divided into two groups, viz:

1st. Para-circular. 2d. Circular.

The first group is subdivided into *para-circular uni-dromes*, *bi-dromes*, *tri-dromes*, or *multi-dromes*, according as they are composed of one, two, three or more disks, in the same manner as for the branched electro-magnets.

Thus the three groups of electro-magnets are composed each of four families. These are subdivided into *genera*, determined by the *number* of helices; into *species*, characterized by the nature of the poles; and finally, into *varieties*, determined by the intensity of the latter.

The number of helices is expressed by the Greek words *monos*, *di*, *tri*, . . . *knemes*, from κνημίς–ίδος, (a greave or leggin,) the nature of the poles by the words *isonomes* or *antinomes*, and their intensity by the words *isodynamic* and *heterodynamic*. The use of these expressions requires some explanation, one fact heretofore unperceived in electro-magnets, one to which at least no importance has been attached, is that we use this apparatus without inquiring if the poles have the same or different intensities; and yet we have shown on several occasions:* 1st, that there is a great difference between electro-magnets of two categories and a difference no less great between bifurcated electro-magnets with poles of the same name or poles of different name.† Take for example, the horse-shoe, which has only one bobbin or spool, and which for that reason is called *electro-aimant boiteux*; it has two poles of unlike names, but these two poles are of different intensity; if we apply to this magnet the above nomenclature we shall have a *bifurcate monokneme* electro-magnet, with *antinome* and *heterodynamic* poles which consequently teaches us the properties of this apparatus just as the expression "sulphate of potash" tell us much more of the composition of that ternary than did the *Sal polychrestum Glaseri* of the alchemists.

If the question is in regard to the common horse-shoe magnet we shall say *Bifurcated dikneme*, for the circular magnets with three disks before described;† then we shall call them *tridome dikneme;* that of the two disks‡ we shall call *bidome dikneme* with isodynamic poles or with heterodynamic poles according as the helix is placed symmetrically or otherwise.

* This Journal [2], xv, 107 and 383. † Vol. xvi, p. 110. ‡ Vol. xx, p. 101.

This very simple classification suggests the kindred simplicity of symbolical notation in chemical compounds. Space fails us to pursue this question farther, as well as many others agitated in this work in which the theoretical points particularly discussed are the following:—

Of isodynamic and heterodynamic poles—Of anomalous poles (*points conséquents*)—Of magnetic phantoms—On the power of electro-magnets as affected by 1st, the elongation of the legs, 2d, the position of the helices, 3d, the distance between the poles—Of armatures—Of the form of polar surfaces—New process for measuring the effective power of electro-magnets—Of magnetic adhesion—Magnetization of locomotive wheels.

The latter process, which we have already described in this Journal (vol. xvi, [2], 337,) has since been successfully repeated in the United States, as we learn from a paper read by Mr. Blake at the Am. Sci. Assoc., 1859.

This work is illustrated by five large plates.

Nancy, Aug. 20, 1860.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *Chemical Analysis by Observations of Spectra.*—It is well known that many substances when introduced into a flame, possess the property of causing in the spectrum, certain bright lines. Bunsen and Kirchhoff have based upon these lines a method of qualitative analysis which materially extends the domain of chemical reactions, and leads to the solution of many difficult problems. In the present memoir, the authors develop the method for the metals of the alkalies and alkaline earths.

The lines in question become more distinct the higher the temperature and the less the specific illuminating power of the flame itself. Bunsen's gas lamp which gives a flame of very high temperature and very low illuminating power is therefore peculiarly adapted to these experiments.

The apparatus employed by the authors is sufficiently simple and does not require a large apartment for its successful use. It consists essentially of two telescopes and a hollow prism filled with bisulphid of carbon, and having a refracting angle of 60°. The eye-piece of one of the telescopes is removed and a metal plate substituted having a narrow slit which is placed in the focus of the objective lens. The lamp is placed before this slit, so that the border of the flame lies in the axis of the telescope. The substance to be examined is placed in a little loop on the end of a fine platinum wire, and is supported in the flame, a little below the point where this is intersected by the axis of the telescope. The rays diverging from the slit are made by the objective to fall upon the prism, and after refraction are received upon the objective of the other telescope the magnifying power of which is about four. The slit should be so wide that only the most distinct of the dark lines in the spectrum are visible. The lower part of the prism carries a small mirror: a telescope directed towards this mirror permits the observation of the image of a horizontal scale placed at a short distance. By turning the prism, the whole spectrum of the flame may be made to pass by the vertical wire of the observing telescope, and every part of the spectrum to correspond with this

wire. A reading of the scale is to be made for each position of the spectrum; this reading however is not necessary for those who know the particular spectra by repeated observation.

Bunsen and Kirchhoff show in the first place, that the different states of combination of the metals examined, as well as very great differences of temperature in the flames produced, exert no influence on the position of the spectral lines corresponding to the particular metals. The same metallic compound gives a spectrum which is the more intense the higher the temperature of the flame; moreover, the most volatile compound of any particular metal always gives the greatest intensity of light.

When small pieces of potassium, sodium, lithium and calcium, are attached to the extremities of fine platinum wires enclosed in glass tubes and the spark of a Ruhmkorff's induction-apparatus is allowed to pass from one pole to the other—the spectra are found to contain the same bright lines as the flames. From this, it appears that these bright lines may be looked upon as certain indications of the presence of the metals in question. They serve as reactions by which these substances may be recognized more sharply, more quickly and in smaller quantities, than by any other analytical process.

The authors give in their memoir colored drawings of the spectra produced by the flames of the six alkaline and earthy metals, together with the pure solar spectrum for the sake of comparison: for these however, we must refer to the original memoir. The special results of the investigation, are as follows:

Sodium.—The spectral reactions of sodium are the most delicate of all. The yellow line Naα the only one found in the sodium spectrum corresponds to Fraunhöfer's line D, and is remarkable for its particularly sharp definition and its extraordinary brilliancy. If the temperature of the flame is very high, and the quantity of substance employed very large, traces of a continuous spectrum are observed in the neighborhood of the line. For this reason, faint lines in the spectra of other substances only become visible, in many cases, when the sodium reaction begins to fade away. The reaction is most distinct with the oxygen, chlorine, iodine, and bromine compounds, as well as with the sulphate and carbonate, but is also distinctly seen in the silicate and other non-volatile salts.

By volatilizing a known weight of soda in a room, the dimensions of which were known, and observing the effect produced upon a non-luminous flame, Bunsen and Kirchhoff have shown that the optical reaction is sufficiently delicate to detect less than the $\frac{1}{3,000,000}$ of a milligramme of soda. It will easily be conceived from this that the atmosphere almost always gives a more or less distinct sodium reaction. The authors suggest that daily and long continued spectral observations may possibly show a connection between the presence and distribution of endemic diseases, and the quantity of sodium in the atmosphere, since chlorid of sodium is an antiseptic substance.

From the inconceivable delicacy of the sodium reaction, it will readily be understood that substances which have been in contact with the atmosphere even for a short time, rarely fail to produce the characteristic spectral line. Striking a dusty book for example produces at a distance of several steps the strongest sodium reaction.

Lithium.—The ignited vapor of lithium compounds exhibits two sharply defined lines, a very faint yellow line Li α, and a brilliant red line Li β. The former lies between Fraunhöfer's lines, C and D, but nearest to D—the latter lies between B and C. The reaction is somewhat less sensitive than that for sodium, perhaps because the eye is more sensitive to yellow than to red rays. The authors find that less than $\frac{9}{1,000,000}$ of a milligramme of carbonate of lithia can be detected with the greatest certainty.

Minerals containing lithia like triphyllin, triphane, petalite, etc., require only to be held in the flame in order to give the most intense line Li α. In this manner the presence of lithia in many feldspars may be immediately detected. Direct observation fails however to detect lithia when present in very minute quantities in natural silicates. In such cases the following process may be adopted: a small portion of the substance is to be digested and evaporated with fluo-hydric acid or fluorid of ammonium, the residue is to be evaporated with a little sulphuric acid and the dry mass extracted with absolute alcohol. The alcoholic solution is to be evaporated to dryness and again extracted with alcohol, and the solution evaporated on a flat watch glass. The residue is to be scraped up with a knife and introduced into the flame by means of the platinum wire: $\frac{1}{10}$ of a milligramme is usually quite sufficient for the experiment.

This process places beyond a doubt the unexpected fact that lithia is one of the most universally diffused substances in nature. The authors have detected it in sea-water, in the ashes of seaweeds, and of various species of wood growing on granitic soils, as well as in many minerals and in spring waters. A mixture of volatile soda and lithia salts gives the reactions of lithia with scarcely diminished distinctness. In consequence of the greater volatility of the lithia salts, the sodium reaction usually lasts rather longer than that of the lithia. In order therefore to recognize the presence of very small traces of lithia, when mixed with soda, the test should be placed in the flame while the observer is looking through the telescope: the lithium line is then often observed only for a few instants, among the vapors which first arise.

Potassium.—Potassium compounds give in the flame a very extended continuous spectrum, which exhibits only two characteristic lines—one at the extreme outer border of the red rays, Ka α, exactly corresponding to the dark line A of the solar spectrum, the other Ka β far in the violet, also corresponding to a dark line in the solar spectrum between G and H, but much nearer to the latter. A very faint line corresponding to Fraunhöfer's line B is only visible when the light is very intense, and is not very characteristic. The blue line Ka β is rather faint but as well adapted as the red line to the detection of potassium. The position of the two lines near the limits of the visible rays renders the reaction less delicate; only about $\frac{1}{1,000}$ of a milligramme of potassium can be rendered visible in this manner.

All the volatile compounds of potash exhibit this reaction: silicates and other fixed salts only yield it when the quantity of potash is very large. When the quantity of potash is small, it is only necessary to fuse the substance with carbonate of soda, in order to produce the characteristic lines. To detect extremely slight traces of potash, the silicate must

be gently ignited upon a platinum crucible with a large excess of fluorid of ammonium and the residue introduced into the flame by the platinum wire. In this manner, it is found that nearly all silicates contain potash. The presence of salts of lithium and sodium do not sensibly affect the reaction; thus it is sufficient to hold the ashes of a cigar in the flame before the slit, in order to detect immediately the yellow line of sodium and the two red lines of potassium and lithium.

Strontium.—The spectra of the alkaline earths are much less simple than those of the alkalies: that of strontium is specially characterized by the absence of green lines. Eight lines are very remarkable in this spectrum—six red, one orange, and one blue. The orange line Srα which lies close to the sodium line, the two red lines Srβ and Srγ and the blue line Srδ are most important from their position and their intensity. The authors find that $\frac{6}{100,000}$ of a milligramme of chlorid of strontium may be detected by the spectral reaction. In order to detect strontium in its less volatile compounds, as in sulphates, silicates, etc., it is best to fuse the substance to be tested, with carbonate of soda, and to moisten the fused mass with muriatic acid. The authors give a very simple and elegant process for effecting the fusion in a spiral of platinum wire, for which however, we must refer to the original memoir. The reactions of potassium and sodium are not disturbed by the presence of strontium. The lithium reaction is also distinctly seen when the proportion of lithium to strontium is not too small.

Calcium.—The Calcium spectrum may be readily distinguished from the four spectra hitherto described by the presence of a highly characteristic and intense line in the green, Caβ. The very intense orange line, Caα, which lies considerably farther toward the red end of the spectrum, than the sodium line, Naα, or the strontium line, Srα, is not less characteristic.

Kirchhoff and Bunsen find that $\frac{6}{100,000}$ of a milligramme of chlorid of calcium may be easily and certainly recognized by the spectral analysis. The volatile compounds of calcium exhibit the reaction with the greatest distinctness: sulphate and carbonate of lime give the spectrum, as soon as the salt begins to become basic.

The compounds of calcium with fixed acids are indifferent in the flame. Those which are attacked by muriatic acid may be advantageously treated as follows: A small quantity of the finely pulverized substance is introduced on the platinum wire into the less hot portion of the flame, until the powder packs together without fusing. A drop of muriatic acid is then added, so that the greater part of it remains in the loop of the wire. On bringing this drop into the hottest part of the flame, it evaporates without boiling, and at the instant that the last portions are evaporated, a brilliant calcium spectrum is seen.

Those silicates which are not attacked by muriatic acid, are most advantageously treated by heating the very finely pulverized substance on a flat platinum crucible cover with about a gramme of half deliquesced fluorid of ammonium, and igniting after the volatilization of the latter. The residue is then to be moistened with one or two drops of sulphuric acid, and the excess of this expelled by heat—the substance thus prepared is then to be introduced into the flame. When potassium, sodium, lithium

and strontium are all present together, the characteristic reactions of the alkalies appear first; those of calcium and strontium usually somewhat later. When these last are present only in very minute quantities, their spectral reactions do not appear; we obtain them however, immediately when the wire is moistened with muriatic acid, and held for a few moments in the reducing flame.

The authors point out the importance of these simple processes in a geological and minerological point of view. Thus sea-water is easily shown to contain potassium, lithium, calcium, and strontium, and many other very interesting and important examples are given.

Barium.—The spectrum of Barium is the most complicated of all those yet investigated. It contains two bright green lines between the spectral lines E and F but nearer to E; these the authors denote by Ba α and Ba β. A third line Ba γ is less sensitive, but still characteristic. The spectral reaction of the barium compounds is somewhat less sensitive than that of the metals already considered: about $\frac{1}{1,000}$ of a milligramme of barium is exhibited with perfect distinctness. The chlorid, bromid, iodid, fluorid, hydrated oxyd, carbonate and sulphate of barium give the reaction immediately on heating in the flame. Those silicates which are attacked by muriatic acid, must be treated in the manner pointed out for the silicates of lime. Other silicates should be fused with carbonate of soda, as in the case of strontium. In a mixture containing chlorids of all the six metals and at most $\frac{1}{10}$ of a milligramme of each, each metal was readily detected by the spectral analysis.

In conclusion, the authors point out the great advantages of the new method of qualitative analysis in detecting the presence of minute quantities of particular substances and in tracing their distribution upon the surface of the earth. They even venture to assert that there exists a hitherto undiscovered metal belonging to the same group with potassium, sodium and lithium, which gives a spectrum as simple and characteristic as that of lithium. This spectrum exhibited with their apparatus, only two lines—one, a faint blue, which almost corresponds to the strontium line, Sr δ and the other a blue line which lies a little farther toward the violet end of the spectrum and compares in intensity and distinctness with the lithium line.

The spectral analysis promises to furnish also a method of investigating the chemical nature of the atmospheres of the sun and of the brighter fixed stars.

Kirchhoff has shown from theoretical considerations, that the spectrum of an ignited gas is inverted when a source of light of sufficient intensity and giving a continuous spectrum, is placed behind it. In other words, the bright lines are under these circumstances converted into dark ones. From this it appears, that the solar spectrum with its dark lines is nothing else than the inversion of the spectrum, which the atmosphere of the sun would show by itself. The chemical analysis of the sun's atmosphere requires us therefore only to determine what substances introduced into a flame will produce bright lines, corresponding to the dark lines in the sun's light. The authors have verified by direct experiment the above conclusions, and have inverted the bright lines of potassium, sodium, lithium, calcium, strontium and barium. They promise a further extension of

their very beautiful and valuable investigations.—*Pogg. Ann.*, cx, 161, June, 1860.

[NOTE.—The inferences of Kirchhoff and Bunsen with respect to the chemical nature of the sun's atmosphere require two assumptions neither of which is as yet supported by observation. The first is that the body of the sun is intensely luminous and that its spectrum contains no dark lines: the second is that no substances except those of which our earth is composed and with which we are acquainted exhibit in their spectra bright lines corresponding to the dark lines in the solar spectrum as we see it.—W. G.]

2. *On some numerical relations between the densities and equivalents of certain elements.*—PLAYFAIR has communicated to the Royal Society of Edinburgh some remarkable observations on the densities of several of the elements. The numerical relation which the author has detected amounts simply to this—that the densities are in certain cases accurately represented by the square roots, cube roots, or fourth roots of the equivalents. In obtaining the densities, the author takes a mean of all the best recorded observations in each case. The following table exhibits his results which are certainly very striking, the coincidences being too numerous and too perfect to be accidental:

	Equivalents.	Density as found.	Density as Calculated.
Diamond,	12	3·48	$\sqrt[2]{12} = 3{\cdot}46$
Graphite,	12	2·29	$\sqrt[3]{12} = 2{\cdot}28$
Charcoal,	12	1·38	$\sqrt[4]{12} = 1{\cdot}36$
Silicon, (adamantoid,)	14·2	2·46	$\sqrt[3]{14{\cdot}2} = 2{\cdot}42$
Silicon, (graphatoid,)	28·4	2·33	$\sqrt[4]{28{\cdot}4} = 2{\cdot}30$
Boron, (adamantoid,)	7·2	2·68	$\sqrt[2]{7{\cdot}2} = 2{\cdot}68$
Bromine,	80·0	2·98	$\sqrt[4]{80} = 2{\cdot}99$
Iodine,	127·0	4·99	$\sqrt[3]{127} = 5{\cdot}02$
Sulphur,	16	2·00	$\sqrt[4]{16} = 2{\cdot}00$
Selenium,	80	4·31	$\sqrt[3]{80} = 4{\cdot}31$

It will be seen that there are some inconsistencies in the selection of the equivalents which however only affect the degree of the root to be extracted. It remains to be determined whether the other elements exhibit similar relations.—*Chemical News*, No. 27 and 29, 1860. W. G.

3. *On the Loss of Light, by Glass Shades;* by WM. KING of Liverpool, and Prof. VERVER of Maestrecht; *with a note of additional experiments;* by FRANK H. STORER.* Under date of Feb. 24, 1860, Mr. King writes to the Editor of the London Journal of Gas Lighting, etc., (see vol. ix, p. 111,) as follows: "Sir,—Having recently tried some experiments for the purpose of ascertaining the amount of light lost by the use of various descriptions of glass shades, I thought that the results obtained might prove not uninteresting to some of your readers, more especially as it is a subject of practical importance, and does not seem to have attracted the notice which it deserves.

The following table exhibits the amount of light lost by the use of the various shades therein enumerated:—

* Communicated for this Journal, by Mr. Storer.

Description of shade.	Loss of light.
Clear glass, - - - - - -	10·57 per cent.
Ground glass, (entire surface ground	29·48 "
Smooth opal, - - - - -	52·83 "
Ground opal, - - - - - - - -	55.85 "
Ground opal, ornamented with painted figures, the figures intervening between the burner and the photometer screen, -	73·98 "

As the large amount of light lost by the use of a clear glass shade excited some surprise, a sheet of common window glass was placed between the burner and the photometer screen, when it was found that 9·34 per cent of the light was intercepted, thus confirming the result obtained by the employment of a shade of clear glass.

I may state that the shades were selected from a large number, and great pains were taken to obtain an average specimen of each kind."

It should be mentioned in passing that Verver* had previously called attention to the subject in these words :—" It is not necessary to surround the wicks [of platinum, placed in the flame of the ' water gas,'] with glass chimneys as is done with ordinary coal gas ; on the contrary it is preferable not to employ them, because the chimneys, no matter how well polished or how clean they may be, always absorb a considerable portion of the light which is produced. This loss (*déperdition*) was shown by the following experiment. A burner with twelve jets without any chimney afforded an illuminating power of 6·75 candles ; but on surrounding the wick with a clean and perfectly polished chimney, the illuminating power amounted only to 5·25 candles ; it had consequently diminished 1·50 candles, *i. e.*, 22 per cent." But, as Schilling† has already remarked, this experiment alone does not prove that the whole of the lost 22 per cent was absorbed by the glass of the chimney, since the conditions under which the gas is consumed when a chimney is used must be entirely different from those which obtain when no chimney is employed.

Immediately after the arrival in this country of the journal containing Mr. King's note, I was requested by Mr. W. W. Greenough, Agent of the Boston Gas Light Co., to institute a series of comparative experiments upon this subject. Since the results of these experiments have in the main fully corroborated those of Mr. King it seems but just to this gentleman to give them publicity.

Instead of lamp shades, flat sheets of glass (ordinary window panes) six inches wide by eight inches high, were fitted to a rack of blackened wire which was fastened to the photometer bar (100 inches long) at a distance of three feet from the gas light.‡

The gas employed was prepared, from the caking coal of Pictou, N. S., expressly for these experiments and was contained in a special gas-holder.

* *L'Éclairage au Gas à L'Eau à Narbonne et L'Éclairage au Gas Le Prince, examinés et comparés par le* Dr. B. Verver, *Prof. de Chimie et de Physique à l'Athénée Royale de Mæstricht.* Leide, 1858, p. 26.

† In his *Journal für Gasbeleuchtung, etc.* München, 1859, ii, 377.

‡ It does not appear that the distance from the source of light at which the glass screen is placed has any appreciable influence upon the amount of light transmitted by it. At all events no such influence could be detected in a number of experiments made purposely to test this question.

The "illuminating power" of this gas when consumed from the Parliamentary Argand burner* at the rate of five cubic feet per hour—as in the following experiments—was equal to 16·00 candles consuming 120 grains, by calculation, (in reality about 135 grains as carefully determined by the balance in each case) of spermaceti per hour.

The experiments were made in the blackened experimental chamber of the Boston Gas Light Co., with the photometer (Bunsen's) which I am accustomed to employ in my daily tests of the gas furnished by the Company to consumers, every precaution being taken to ensure accuracy. It may also be mentioned that none of the measurements (of the distances of the photometer, from the standard candle) obtained by actual experiment were calculated,—*i. e.*, reduced to their equivalents in candles,—until the whole series of experiments was completed, and that no comparison of my own results among themselves or with those of Mr. King was made until each member of the table had been calculated as it stands below. Whatever the experiments may be worth therefore, they have at least the merit of being entirely independent and wholly unbiassed.

Description of glass.	Thickness of glass.		Loss of light.
Thick English plate,	$\frac{1}{3}$ of an inch,		6·15 per cent.
Crystal plate,	$\frac{1}{8}$ "	- -	8·61 "
English crown,	$\frac{1}{8}$ "	- -	13·08 "
"Double English," window glass,	$\frac{1}{8}$ "	- -	9·39 "
"Double German,"† "	$\frac{1}{8}$ "	- -	13·00 "
"Single German,"† "	$\frac{1}{16}$ "	- -	4·27 "
Double German,† ground,‡	$\frac{1}{8}$ "	- -	62·34 "
Single German,† ground,‡	$\frac{1}{16}$ "	- -	65·75 "
Berkshire, (Mass.,) ground,‡	$\frac{1}{16}$ "	- -	62·74 "
Berkshire enameled, *i.e.*, ground only upon portions of its surface,—small figure,	$\frac{1}{16}$ "	- -	51·23 "
"Orange-colored" window glass,	$\frac{1}{16}$	As used for church windows, &c.	34·48 "
"Purple" " "	$\frac{1}{8}$	As used for church windows, &c.	85·11 "
"Ruby" " "	$\frac{1}{16}$	As used for church windows, &c.	89·62 "
"Green" " "	$\frac{1}{16}$	As used for church windows, &c.	81·97 "
A porcelain transparency, (Tyrolese Hunter,)	$\frac{1}{16}$ "	- -	97·68 "

The term "loss of light" employed by Mr. King does not at first sight seem to be strictly appropriate, for a very considerable portion of the light not transmitted by a glass shade might be reflected against the walls of

* With the ordinary chimney.—Mr. King does not specify what burner was used in his experiments, but it was probably the "fish-tail," which has been so often recommended as a standard by his father, consuming four feet per hour, of course without a chimney.

† Among the Boston dealers, the term German is applied to glass of Belgian manufacture.

‡ The enormous resistance to the passage of light which is offered by ground glass is certainly worthy the attention of those using it for windows, &c.

The discrepancy between Mr. King's results and my own as regards ground glass may perhaps be owing to the fact, that the window glass used by myself was *more coarsely ground* than the lamp shades employed by him.

the apartment in which the lamp is burning and thus aid in the general illumination of the room. The meaning which Mr. King attaches to the term is however so perfectly evident that I have not hesitated to follow him in using it. For that matter, there can be no doubt but that the numbers given by us express as accurately as the circumstances of the case will admit, the actual diminution of the amount of light, falling for example upon the pages of a book held near to its source, which would be occasioned by the interposition of the shades enumerated in our tables.

Boston, April 20, 1860.

[We cannot doubt that the great loss of light proved by the experiments above given, is to be, in part at least, accounted for by the conversion of a portion of the light into heat—an effect perfectly in harmony with the theory of transverse vibrations as applied to explain the phenomena of polarization of heat. On this theory, heat and light are different effects produced by one and the same cause, and they differ physically only in the rapidity and amplitude of their vibrations. The screen through which the vibrations of light are propagated serves to diminish first the rapidity of the vibrations requisite to produce the most refrangible rays, and in proportion as the transparency of the screen is diminished by any cause, inherent or superficial, this arrest becomes more and more complete. As the more rapid etherial vibrations have probably the least amplitude, we infer from analogy in sound-waves, that as waves of least intensity have the greatest amplitude, so with the luminiferous ether the extreme red has but little brilliancy. Hence the loss of light from polished screens is small compared with that observed in screens of opaline or roughened glass. It would be instructive to examine the spectrum obtained from a pencil of rays under each of the cases given, by means of a sulphid of carbon prism.

The subject of absorption of light by screens has long since been carefully examined by *Bouguer.** By a photometric method essentially like Rumford's, Bouguer measured the loss of light in the beam of a candle compared with a flambeaux and also with the light of full moon, in passing through 16 thicknesses of common window glass having an united thickness of 21·43 millimetres = ·85 inch. The mean loss of light shown by these trials was as 247 : 1, or over 99 per cent of the whole quantity.

Six plates of the purest mirror plate-glass having an united thickness of 15·128 millimetres diminished the light in the ratio of 10 to 3, occasioned a loss of about 70 per cent of diffuse daylight. A mass of very pure glass about three inches thick diminished the light only about half the latter amount, owing to its being a single mass and not cut up into many planes.

He also measured the absorbing power of sea-water for light and found as the results of experiments made in France and of observations also in the torrid zone, that at the depth of 31 French feet the light of the sun would be equal only to that of th full moon, and at the depth of 679 feet would wholly disappear. e estimates the transparency of the air as 4575 times greater th that of sea-water, and

* *Traité d'Optiqae sur la Gradation de la Lum ouvrage posthume) : publié* par M. l'Abbé De La Caille. *Paris*, 1760, 4to, pp. .

from the properties of a logarithmic curve (which he calls *gradulucique*) whose functions he had determined experimentally, he seeks to fix the outer limits of the atmosphere. Bouguer was an expert geometer and sustains all his conclusions by mathematical demonstrations. His results seem to have received less attention than they merit, the only reference to his researches I have seen being by Daguin in his excellent *Traité de Physique*, iii, 300, 1859.

We should not omit in this connection to refer to the very interesting observations of Draper* on the spectrum formed by means of a platinum wire heated gradually from dull redness to perfect whiteness by a volta-electric current. He observed the red part of the spectrum to appear first and as the heat and brilliancy of the wire increased the other colors of the spectrum appeared after the violet. This result perfectly harmonizes with views above expressed.—B. S., JR.]

ASTRONOMY.—*New Planets.*—A new planet, the 59th of the group between Mars and Jupiter, was discovered Sept. 15, 1860, by Mr. James Ferguson, of the Washington (D. C.) Observatory. It was then about as bright as a star of the 11th magnitude.—*Gould's Astron. Journal*, No. 140.

The 58th Asteroid (since named *Concordia*) was discovered March 24, 1860, by Dr. Luther of the Observatory at Bilk.

PERSONAL.—Prof. J. D. WHITNEY, State Geologist of California, sailed from New York for his post of duty on the 22d of October last with Prof. WM. H. BREWER (late of Washington College, Pa.) in charge of the Department of Agricultural Chemistry and Botany, and Mr. WILLIAM ASHBURNER who is assistant in the field Geological and Astronomical work. The head-quarters of the survey will be for the present at San Francisco. But the Act authorizing the survey contemplates the establishment of a State museum, on the most extensive scale, the location of which is to be determined by a future legislature, which place, wherever it may be, will doubtless be also the permanent head quarters of the survey. For the present, letters addressed to any member of the corps, "State Geol. Survey, San Francisco," will reach their destination. Parcels or books for Prof. Whitney or any member of the corps may be sent to care of B. Westermann & Co., 440 Broadway, New York. No similar enterprise in the United States has ever been set on foot on a more liberal and enlightened basis, or opened under more favorable auspices as respects either the importance of the work to be done or the ability of those charged with the duty.

THE GENERAL INDEX to the 3d decade of volumes of the 2d Series of this Journal, now complete, occupies more than the space usually appropriated to our Scientific Intelligence, and our numerous friends, whose contributions are thereby excluded from the present issue, will pardon the unavoidable delay.

OBITUARY.—Died in Montreal, Oct. 9, Dr. W. P. HOLMES, well known as an early cultivator of mineralogy and botany in Canada, an active promoter of the Montreal Natural History Society, and for the last ten years professor of Medical Jurisprudence in McGill College, Montreal.

* This Journal [2], iv, 388, and v, 1.

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XXI.—P. 91, l. 9 from top, after 'from' *insert* 'the denominators of.'—P. 159, l. 7 from top, for '0″·6,' *read* '0″·6.'

XXII.—P. 1, l. 2 from bottom, for '*Dictyopyxis,' *read* 'Dictyopyxis; l. 4 from bottom, for 'Dicladia,' *read* '*Dicladia.'—P. 2, lines 1 and 2 from top, for '*Coscinodiscus,' *read* 'Coscinodiscus; l. 2 from top, for 'Rhizosolenia,' *read* '*Rhizosolenia;' l. 8 from top, for 'Difflugia,' *read* '*Difflugia;' l. 16 from top, for 'Eucyrtidium,' *read* '*Eucyrtidium,' in both cases.

XXIII.—P. 88, l. 2 from bottom, for 'northeast,' *read* 'east.'—P. 178, lines 3 and 5 from bottom, for 'Tannenschein,' *read* 'Sonnenschein.'

XXIV.—P. 123, lines 24 and 25 from bottom, *transpose* 'mica pseud.' and 'unalt'd scapolite.'—P. 313, l. 13 from top, for 'equalities,' *read* 'qualities.'—P. 314, l. 21 from bottom, for 'propositions,' *read* 'proportions.'—P. 428, l. 14 from bottom, for 'Loccopteris,' *read* 'Laccopteris.'—P. 443, l. 1 from bottom, for 'JOHN WARREN,' *read* 'JOHN WARNER.'

XXV.—P. 318, l. 23 from bottom, for 'of the south,' *read* 'of the north;' l. 8 from bottom, for 'and separates,' *read* 'separating;' l. 2 from bottom, for 'Nevada,' *read* 'Sierra.'—P. 375, l. 19 from top, after 'water,' *add* 'or metallic oxyd;' l. 18 from bottom, after 'the,' *insert* 'gaseous.'—P. 422, *dele* last paragraph of note.—P. 437, *dele* lines 5 and 6 from bottom.

XXVI.—P. 67, lines 4 and 5 from top, for '$\frac{3}{8}\dot{\mathrm{R}} : 2\frac{1}{3}\dddot{\bar{\mathrm{R}}} :: 4\dddot{\mathrm{Si}} : 1\dot{\mathrm{H}}$,' *read* '$\frac{3}{8}\dot{\mathrm{R}}^3 : 2\frac{1}{3}\dddot{\bar{\mathrm{R}}} :: 4\dddot{\mathrm{Si}} : 3\dot{\mathrm{H}}$,' and *dele* the index 3 after $(\frac{3}{8}\dot{\mathrm{R}}^3 + \frac{7}{3}\dddot{\bar{\mathrm{R}}})$.—P. 108, l. 7 from bottom, for 'J. W. BILL,' *read* 'J. H. BILL.'—P. 158, l. 1 from top, for '95,' *read* '96.'—P. 169, lines 12 and 14 from bottom, for '*v*,' *read* '*V*.'—P. 170, l. 3 from bottom, after 'as much as,' *insert* 'one-fourth of.'—P. 236, l. 17 from top, for '80,' *read* '90.'—P. 298, l. 19 from bottom, for 'inside,' *read* 'irised.'—P. 305, in the title of Hayes's article, *dele* 'with a map.'—P. 331, l. 5 from bottom, after 'the beak of the,' *insert* 'smaller valve in the.'—P. 332, l. 19 from top, after 'identical,' *insert* 'with foreign forms.'—P. 333, l. 10 from top, after 'Cretaceous,' *insert* 'and perhaps Tertiary.'

XXVII.—P. 243, l. 1 from bottom, after 'Delphi Slate,' *insert* 'or Black Lingula shale, equivalent of the Genesee Slate or Marcellus shale of New York.'

XXVIII.—P. 236, l. 1 from top, for 'lightly,' *read* 'slightly;' l. 7, after 'other,' *insert* 'side is a.'—P. 240, l. 1 from top, after 'Actinocrinus,' *insert* 'grandis;' l. 16 from top, for 'obscure,' *read* 'obscurely.'—P. 241, l. 2 from bottom, for 'oval,' *read* 'oral.'—P. 244, l. 14 from top, for 'armbones,' *read* 'umbones;' l. 6 from bottom, for 'cyclostomus,' *read* 'Cyclostomus.'—P. 248, l. 23 from top, for 'p. c.,' *read* 'oz.'—P. 252, l. 28 from top, for '$\frac{1}{5}$,' *read* '$\frac{1}{8}$.'—P. 291, l. 1 from bottom, for 'ENGLEMANN,' *read* 'ENGELMANN.'—P. 353, l. 14 from bottom, for '3·8924+121·80,' *read* '3·8924×121·80;' l. 10 from bottom, for '4·6440+121·80,' *read* '4·6440×121·80.'—P. 356, l. 20 from bottom, after 'cultivation,' *insert* a period, and for 'where,' *read* 'Where;' l. 19 from bottom, *dele* 'then.'—P. 357, l. 10 from top, for 'sand,' *read* 'mud.'—P. 403, (in some copies,) l. 8 from bottom, for 'ANDREAS,' *read* 'ANDRAS.'

XXIX.—P. 272, l. 9 from bottom, for 'HENTZ,' *read* 'HEINTZ.'—P. 295, l. 2 from bottom, for '*Carten*,' read '*Garten*.'—P. 452, l. 11 from bottom, and p. 453, l. 14 from top, for 'LEIBER,' *read* 'LIEBER.'—P. 454, l. 13 from top, (in some copies,) for 'DAGURIN,' *read* 'DAGUIN.'

XXX.—P. 162, l. 16 from bottom, for 'many,' *read* 'some.'—P. 167, l. 23 from top, for 'best,' *read* 'test.'—P. 174, l. 18 from top, for 'point,' *read* 'part.'

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